EXPLANATIONS AND COMMUNICATIVE CONSTRAINTS IN NATURALLY OCCURRING DISCOURSE

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The subject matter of this thesis are some aspects of the expression of explanations in spoken discourse. The study of explanations has occupied the attention of many researchers in social psychology and in neighbouring disciplines; the study of talk has occupied an even greater number. In the thesis I try to integrate certain areas of these two fields. Chapter one sketches the history of the concern with language which has characterised developments in the social sciences this century. This chapter is incidentally an introduction to some of the key themes of the thesis and to why I think research based on naturally occurring discourse is important.

Research on explanations in social psychology has been dominated by research which has gone on under the heading of attribution theory. In chapter two I address a controversy in the application of concepts drawn from attribution theory to clinical psychology, namely whether or not people have fixed styles in the way that they attribute causes for outcomes. Studying family therapy sessions and interviews with parents with a coding procedure I show that the variety of possible styles is broader than has been suggested previously. Chapter three further pursues causal expressions as cases of explanations by asking what a causal statement is. The chapter opens with a discussion of how causes relate to reasons concluding that reasons are a species of cause. I then go on to use data from earlier work to study what expressions speakers use to make causal utterances.

The direction of enquiry has been to suggest that rather than studying causal beliefs it is causal utterances that are under study. An utterance is, if you like, "situated", that is to say, what a speaker says is context-bound. I talk of "communicative constraints" operating here. Chapter four reviews some work in the study of conversation with an eye to elucidating the sense in which a speaker's utterances are a product of the situation in which they occur and to look at the researchability of this intuition. Practical and conceptual reasons suggest that the approach generally known as conversation analysis stemming from the study of ethnomethodology is the most interesting and fruitful way to

proceed (in this context). Chapters five and six report studies of a computing advisory centre showing 1, the range of accounting procedures which occur as part of the business-at-hand in these sessions, 2, how speakers' utterances can change within a single conversation. Chapter six looks at the integration of non-vocal behaviour and by considering data on this argues that the idea of normativity, rather than a quasi-grammatical notion, is the appropriate level of explanation for the regularities which we find in human interaction.

In moving away from beliefs as the object of analysis I could be accused of taking an anti-cognitive stance. Chapter seven explores cognitive versus interactional perspectives in communication.

Chapter eight reflects on the approach which I have adopted and suggests how inspite, indeed through, its focus on situational events an account of the capacities drawn on in offering explanations can itself illuminate phenomena seen as beyond its grasp.

In carrying out the research discussed here I have incurred debts to many people. It is a pleasure to thank Peter Stratton for his guidance over the last three years. I am grateful to the Leeds Family Therapy and Research Centre and to the people who have let their talk in it be used for research. Special thanks to Helga Hanks and Jo Silvester. Thanks too to the Computing Services for helping me with my request to video record advisory sessions there. I am grateful to the advisors and users who let me record their work. My thanks also go to the University of Leeds. Six years based here are now coming to an end for me.

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I've tried hard to write something worthy of being dedicated to someone. I don't know whether or not this has happened. If anything here is found interesting or enjoyable then I'd like to dedicate that, with love,
to the memory of my mother and to my father.

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INTRODUCTION: SPOKEN EXPLANATIONS

1.0 Outline

This work has been motivated by an interest in communication and interaction. I am fascinated by the commonplace human capacity to make sense of new situations, to produce and communicate explanations. Here my focus is on explanations which occur in talk. I have used the term "discourse" in preference to "conversation" because despite the currency of this as a general term for stretches of people talking to each other, I feel that outside sociological work the term connotes a specifically casual kind of interaction. "Discourse" on the other hand, has the disadvantage, for me here, of covering spoken and written stretches of language - I restrict myself to spoken discourse. The term does have connotations of argument (in the sense of reasoning), as comes out in the adjective "discursive". This is convivial, since as I hope to show, it is the rational basis of talk which makes communication, and analysis of it, possible. I shall not attempt to defend explanation as a research topic in its own right and I shall not be concerned with developing a typology of explanations (blamings, excuses, defences, warrants and so on).

An explanation, if it is successful, makes something plain. In the case of someone explaining something to another this implies both the presence and absence of intersubjectivity. Absence because some party can see something another can't, or at least thinks they can. Explanations deserve attention because in them a speaker attempts to lead a recipient to see something in a certain way. To accomplish this a speaker must have command of a capacity which is as specific in its operation as it is difficult to describe. Ethno-methodologists gloss it as "practical reasoning". An emphasis on practical, situated reasoning is the hallmark of a variety of approaches to the problem of social order in sociology and anthropology. Studying the social structure of conversation and studying speech patterns in relation to social relations make up part of an identifiable set of research interests. These approaches, together with others concerned with studying the meaning that people assign to the objects and events in their worlds, have been glossed as "micro-sociologies". Such work

shows a concern with human behaviour as a situated phenomenon. In chapter one I review such approaches, trying to show how they differ from each other and from social psychology. The idea of "rule following" occurs as a controversial topic in these accounts. I try to distinguish the different ways in which this idea has been invoked and the sense that can be made of its application to the analysis of social behaviour.

Part of the business of chapter one is to introduce the Leeds Attributional Coding System (LACS) (a way of describing causal explanations) and attribution theory in general. This was the starting point for the research discussed here. In keeping with the dominant tradition in psychology, and in social psychology in particular, attribution theory has been explored overwhelmingly through experimental procedures. I draw a sharp distinction within empirical work between observational and experimental research. By empirical work I mean research based on experience, as opposed to theoretical or conceptual work. An experiment is a procedure whereby a situation is contrived where things are played about with so that the effects of different things can be distinguished. Observational research lacks the element of manipulation. The LACS was designed specifically to research causal beliefs in naturally occurring discourse and to this end used some of the concepts developed in attribution theory independently of some of the models which have been proposed. In fact there is a tradition of using such concepts in clinical psychology from which the LACS takes its lead. The significant difference is that the LACS takes naturally occurring discourse as its subject matter.

The rationale for the LACS and the research based on it here at Leeds is that causal beliefs are manifestly an important topic but that research based on them in the clinical area has generally been carried out by indirect means, that is by questionnaire, and has been carried out on undergraduate students. An interest in naturally occurring discourse is a natural development of a preoccupation with the beliefs that caregivers, parents and family members have about each other, particularly in cases of distress and sexual and physical abuse, especially given that these issues are discussed in therapy sessions. I have deliberately presented the LACS in the context of approaches to exploring how people make

sense of the world, rather than via a discussion of attribution theory itself or the psychology of the family.

The thesis contains two pairs of chapters reporting empirical work. The first pair of these asks simple questions in relation to causal explanations. The three studies in chapter two ask whether or not there are attributional styles. Chapter three looks at an issue in the methodology used in chapter three: what is it for an utterance to be an expression of a causal belief? Both these chapters start with introductions which are quite lengthy discussions in their own right on attributional style and on the cause-reason distinction. Although there is an argument for separating out the conceptual and empirical business I have presented the two in tandem for two reasons. Firstly this is the way I like to work. In each of these chapters the empirical and conceptual work belong closely together; each chapter addresses a fairly self contained problem.

The situated nature of explanations has, so far, not been discussed. One objection to the kind of research carried out through an instrument such as the LACS is that what people are saying might be affected, or somehow distorted by the pressures of the situation. Far from seeing this as a problem I see it as something of great interest. Chapter four picks up the issue of how spoken discourse itself can be analysed. For my purposes the approach generally known as conversation analysis is adopted. It should be pointed out that this approach is hardly a methodology, in the sense of a body of methods: it is an orientation not a set of procedures. I introduce the idea of a communicative constraint as the most general term to describe the sense in which an utterance is tied to its context, drawing together various ways in which different writers have used the idea of "constraints".

Chapters five and six study communication conduct in computer advisory sessions. Drawing on the conversational analytic literature and on the transcription method developed in it I study talk in these sessions. I show how explanations of the user's problem and of the advisor's answer are collaborative endeavours, how speakers change the expressions that they use, and how account elements crop up as part of the business of the session. It is a commonplace

that nonverbal behaviours are an important aspect of communication conduct. In chapter six I note some body movement and gaze phenomena. This is a departure from the topic of explanations but findings are reported because they demonstrate the sense in which participants' activity can be described in terms of norms and resist a quasi-grammatical account.

Explanations have commanded the attention of a number of researchers. Austin's (1961) philosophical analysis of excuses should be noted as an attempt to grasp the logic of excusing. Scott and Lyman (1968) look at explanations of untoward behaviour. There has also been interest within social psychological work on causal beliefs in particularly in Antaki's work. Antaki (1988) contains a dozen approaches to looking at explanations in a number of senses. Amongst these is a paper in the tradition of conversation analysis by Heritage. Several pieces of work in this tradition have related to explanation, for example, Pollner (1979) "Explicative transaction: making and managing meaning in traffic court", Twer (1972) "Tactics for determining person's resources for depicting, contriving and describing behavioral episodes", Pomerantz (1978b) "attributions of responsibility blamings".

The closing chapters address problems and issues that seem to me to be of special interest. I want to close this introduction with some preliminary remarks explaining some of the relevant terminology.

2.0 Conventional distinctions in analysing language in use

2.1 Utterances and inscriptions

There are clearly important and obvious similarities between the analysis of stretches of written language and of spoken language. At the same time there are equally clear and important differences. At one level, the analysis of utterances becomes the analysis of texts as it is transcribed material that is studied. Wetherell (1986) and Parker (1988) are examples of the value of a literary critical approach to social psychological issues. Mulkay (1985) has suggested ways in which ideas from the analysis of conversation can be related

to textually mediated communication, such as letter writing. As I briefly mention in chapter eight, Freud suggested a theory of interpretation which although primarily drawn on in analysing talk in the psychoanalytic situation, claims application to a variety of artifacts and behaviours, such as dream reports and parapraxes. Here I shall restrict myself to approaches developed primarily with an interest in analysing talk.

2.2 "discourse analysis"

The history of this term could itself be the subject of an interesting study of the emergence of standards amongst groups of researchers. I have already discussed my use of the term "discourse", and I use "discourse analysis" to mean the analysis of that phenomenon in general. This is the line taken by van Dijk in the title of his Handbook of Discourse Analysis. Potter and Wetherell (1987), remark that it is possible to find two books on discourse analysis with no overlapping content. They follow Gilbert and Mulkay (1984) in representing discourse analysis as something delimited, and in practice meaning something quite narrow. As far as I know the term was originally used by Zellig Harris:

> One can approach discourse analysis from two types of problem, which turn out to bé related. The first is the problem of continuing descriptive linguistics beyond the limits of a single sentence at a time. The other is the question of correlating 'culture' and language (ie non-linguistic and linguistic behavior). (1959, p. 1)

Harris's parenthetical gloss of the second problem seems to be a third issue, in any case, Harris had very little to say on non-linguistic behaviour or on culture, and most discussions (for example Taylor and Cameron, 1987) omit that Harris had an ambition apart from the extra-sentential one. As Taylor and Cameron show, a version of the application of linguistics to speech beyond the level of the sentence remains the intellectual motivation of several of the accounts of conversation that we will consider. But it is generative linguistics, and the model that instances of language are generated by rules, that such analysts turn to.

2.3 Pragmatics

Although this term has a much older history it is convenient to raise Charles Morris's use of it. In defining "semiotics" as the study of sign systems, which is more comprehensive still than even the broadest sense of "discourse analysis" (since it would include non-human communication as well as pictorial communication) Morris (1938) suggested a three fold distinction, syntactics (or more usually syntax) semantics and pragmatics. Whilst syntax would study the relationships between signs and semantics the relationships between the signs and the things that they apply to pragmatics took as its concern "the relations of signs to interpreters". Although Morris later modified this definition to relate to "the origins, uses and effects of signs" (Morris, 1946), Carnap's use of the earlier position promoted the idea of pragmatics as the study of language with explicit reference to its users (Carnap, 1942).

In most English language texts pragmatics thereby assumes a fairly narrow, though not necessarily precise meaning, namely, the study of utterance interpretation. In this sense, speech act theory, with its conceptual analysis of what conditions need be met for a certain utterance to be successful, and by extension, what is implied by the performance of such an utterance, is a paradigm pragmatic line of enquiry. Although as Searle, Kiefer and Bierwisch (1980) point out, the leading theorists in the field, Austin, Grice and Searle himself do not use the term. In a tradition associated with Wittgenstein the meaning of some expression is analysed in terms of its use, thus eroding the distinction between pragmatics and semantics. For Searle the illocutionary force of an utterance is part of its meaning, a pragmatic component cannot be separated out from one of literal meaning, so there is no autonomous concern for the label "pragmatics" to attach to.

Generally, however, pragmatics has a vague meaning, inevitably perhaps since however broad the spectrum of human activity language reflects it. As Levinson (1983) notes in the introductory chapter of his classic book, this breadth is drawn on in the communication theories of psychopathology emblematically represented by Watzlawick, Beavin and Jackson (1967), The pragmatics of

human communication. In looking at spoken explanations I shall, of course, be concerned with the use and effects of signs. I shall leave the issue of how discourse itself may be analysed until chapter four, where I will pick up discussion of Searle's work. First I want to look at some of the ways in which discourse has struck certain social scientist as a research topic. I will then focus on one way in which explanations have been conceptualised and researched.

Given the ubiquity of speech as concern it is not surprising that there are many different approaches to the analysis of it. I don't feel that any overview of the whole field is helpful. Taylor and Cameron, who present their study of the field as the first critical one achieve economy and cohesion by showing that the six approaches that they consider all invoke the same fundamental idea of conversational units organised by rules. I think that their insightful and bold discussion places great strain of the notion of a rule. I do however agree that the search for units in talk is riddled with insurmountable problems.

CHAPTER 1: THE STUDY OF DISCOURSE AND THE SOCIAL SCIENCES

A number of disciplines in the social sciences have made day-to-day things and processes of social life their principle interest. This has, perhaps inevitably, involved a significant orientation to considering the medium through which meaning is expressed: language. Particularly, stretches of language use - discourse - has been a research topic of fundamental importance. Reviewing such approaches I find a number of ideas which have occurred elsewhere in the study of language, for example, the idea of rule-following is thematic. Diversity in methods and theory are also to be found. I attempt to apply issues emerging from a discussion of these approaches, their formulations of human understanding, the construction of a social reality, to inform non-experimental research in the social psychology of explanations.

1.0 Introduction

Since using language is an important human activity it would be surprising if a lot of effort in the humanities and the social sciences had not been given up to exploring, describing, puzzling over and studying it. Looking at these disciplines today, there seem to me to be three themes which characterise their orientation to language: language as a topic of study; language as a resource -an index to something else; and language, or the fruits of other studies of language, as a model for understanding some area of human experience. As examples of these, I have in mind, respectively, such diverse things as: Chomsky's work on syntax; anthropological studies of family organization through the study of kinship terms; and Levi-Strauss's approach to anthropology inspired by de Saussure's structural-ist linguistics.

A mixture of these themes can be found in the approaches to exploring the meaning which people attach to the phenomena that they encounter. Knorr-Cetina (1981) lists a number of approaches to studying what she calls the "micro-processes" of social life. By this she means issues such as: 1, face-to-face interaction; 2, everyday routines and classifications; 3, strips of conversations and 4, definitions of self and situations. Since these phenomena which these

refer are, I guess, for most members of society the processes of social life the "micro" prefix requires explanation. Of course it is in contrast to the traditional business of sociology, analysing and explaining at the level of aggregate action, that these phenomena are "micro" ones. The approaches, together with the representative texts offered by Knorr-Cetina, are:

symbolic interactionism: Blumer (1969), with a book of that title;
cognitive sociology: Cicourel (1973), with a book of that title;
ethnomethodology: Garfinkel (1967), Studies in ethnomethodology;
social phenomenology: Berger and Luckmann (1967), The social construction of reality;
ethogenics: Harre and Secord (1972), The explanation of social behaviour;
the ethnography of speaking: Hymes (1974), Foundations in sociolinguistics: an ethnographic approach;
and ethnoscience: Sturtevant (1964), "Studies in Ethnoscience".

Knorr-Cetina refers to these approaches as "micro-sociologies". A convenient label, but one that I have reservations about. Firstly, the term rather begs the question that sociology has a certain subject matter, of which these are specialisations. This has been resisted by students of ethnomethodology (Sharrock and Atkinson, 1986, p. 118). Secondly, the term masks the difference between these approaches. Furthermore the approaches are on different footings. Whilst "ethogenics", "ethnoscience" and "the ethnography of speaking" present themselves as new ways of doing research, symbolic interactionism and particularly, social phenomenology attempt theoretical formulations of basic features of social life. For this reason I shall not say much about these approaches. Ethnomethodology draws on ideas from social phenomenology or more generally, phenomenological sociology, and I will not discuss this approach independently. To be sure, Knorr-Cetina is doubtless aware of all this, and indeed what these research endeavours arguably do have in common is that they can be construed as positions which are as antagonistic to methodological collectivism as they are to methodological individualism.

Methodological individualism holds that the elements of a social theory should be the beliefs and desires of individuals that make up society. Against this, methodological collectivism appeals to aggregate phenomena for its level of explanation. In fact Durkheim was keen to develop an autonomous area for sociology as a new science to operate at this level. His study of suicide is a classic piece of work in this tradition. The "micro-sociologies" are antagonistic to both positions in so far as they stress situated interactions as places of prime importance: not isolated individuals nor survey data (at a distance from the time and place and procedures where it was collected) but individuals in interaction.

Ethnomethodologists, who to anticipate my discussion are the most theoretically refined, have been quick to point out how their work differs from other approaches to "the sociology of everyday life". (I have reservations about the term "everyday life" partly because the data which I'll consider in chapters two, three, five and six, wouldn't be considered "everyday" by most people. Moreover, the term appears to be question begging: what is everyday life? For whom and when?) Thus Heritage (1984), in his study of Garfinkel and ethnomethodology, (p. 2-3), views the fact that Studies in ethnomethodology appeared at a time when dissatisfaction with structural sociology was commonplace meant that the theoretically distinctive character of the work was lost. Certainly, though these approaches are all fundamentally concerned with meaning. I think the differences between them are highly informative and that they merit analysis. Unlike Knorr-Cetina I am not concerned here with the relationship between these approaches and what she calls macro-sociology, but more on this in chapter eight.

In this chapter I propose to 1, discuss some ideas, drawn from linguistic and philosophical work on language, which are important themes in the work which I shall be examining; 2, develop an account of the approaches which Knorr-Cetina calls "microsociologies" in order to 3, contrast their orientation to language with a dominant social psychological approach to people's understanding of everyday phenomena.

2.0 Ideas about language as a source for ideas about human action

I want to start with a consideration of some important conceptual ideas derived from consideration of language; the "etic-emic" distinction and the concept of a rule.

2.1 The "etic" - "emic" distinction

A convenient distinction which occurs in the literature on studying ways of understanding, and in the analysis of conversation (see Chapter 4 below) is the emic - etic distinction drawn by Pike (1964) to refer to two standpoints for describing behaviour. An emic approach is one that studies behaviour within the "system", as Pike calls it, under study. The etic viewpoint studies behaviour "as from outside of a particular system, and as an essential initial approach to an alien system." (p. 37). The origin of the terms is in the suffixes of the two approaches in phonology, the study of the sounds in a language, namely phonetics and phonemics. Whereas the study of phonemes is the study of significant sounds in a given language, phonetics is the study of all speech sounds prior to their interpretation in a system. The idea here is something like this: Due to physiological constraints on the human vocal apparatus all possible speech sounds can be described and identified by a phonologist. This yields an, in principle, exhaustive scheme onto which the limited range of speech sounds contingently occurring in a natural language can be mapped.

2.2 Chomsky and Wittgenstein on Rules

The idea that language, behaviour, and even emotions are in some sense rule governed, or involve the use of rules, or can be explained or described in terms of rules is a pervasive one. It is an idea that requires careful understanding. The hegemony of the rule idea is undoubtedly attributable to two hugely influential books: Wittgenstein's Philosophical Investigations and Chomsky's Syntactic Structures. These two writers, however, use the term in different senses.

Chomsky's problem is to explain how a speaker of a language knows whether or not an expression is grammatically well-formed. The problem clearly cannot be answered by any notion of recognising a grammatical sentence from a finite stock because there are uncountably many possible sentences. Chomsky's solution is to show that a finite set of rules can generate a variety of grammatical forms.

In what sense do speakers know these rules? Well clearly they can't articulate them - describing them was an immense intellectual endeavour that had to await Chomsky. The point is rather that these rules are a model of how speakers do what they do. The situation is analogous to the relationship between a theory of distance-perception based on the angle subtended on the retina of the eyes and road crossing. Modelling distance judgement in this way might lead us to talk of pedestrians, or their brains, carrying out trigonometric calculations, but we would distinguish this from the formal ability to cope with trigonometric problems in a school mathematics lesson. In fact it is worth remarking that a whole discipline, psycholinguistics, developed taking as its rationale the exploration of the psychological reality of Chomsky's grammar.

We may talk of such rules as licensing certain configurations of words. In an analogous way the rules of chess permit certain, but not any, configurations of pieces on the board.

Wittgenstein's idea of following a rule is related in so far as a person said to be following a rule in his sense, as in Chomsky's sense, would be unable to state what the rule was. Moreover, the rules in question may not be stateable at all, and whereas the proof of the pudding for Chomsky is in stating the rules in question this is in no way Wittgenstein's ambition. It is important to emphasise this because some approaches (eg Harre's ethogenics) which have their origin in the idea that social life is rule governed seek to uncover and state such rules. What then is the point of a rule in Wittgenstein's analysis? A deep concern of the Philosophical Investigations is with what it is for words to have meanings. As is well known Wittgenstein sets up the idea that we can consider the use to

which words are put by speakers and that we do not need to appeal to any kind of mental phenomena in doing this. To get a purchase on how words can have a meaning in this sense Wittgenstein suggests that we consider language as analogous to a game. Within a game actions, movements, noises and sundry goings-on have an importance which is closely bound up with the systemic nature of that game. Certain things are integral to a game and outside that game could be meaningless or mean something else altogether. This analogy, between a language and game, is explored through the development of the idea of a language game. In connection with a game it is pertinent, indeed fundamental, to think of the significance of moves in terms of the rules of the game and to say that a competent player has mastered the rules. Wittgenstein presses the analogy: in a language too, let us consider the significance of words, that is their meaning, as being connected with rules. Wittgenstein interrogates this idea with immense subtlety. What I think is important for our purpose is the following. The rules are followed blindly. We do not know the rules in the sense that we cannot articulate them. We simply (simply!) have the capacity to grasp the meaning of words, definitions are not exhaustive. If the rules were not followed blindly we would need other rules to interpret them.

2.2.1 Regulative and constitutive rules

Another important distinction to be drawn in connection with the concept of a rule is the distinction between constitutive rules and regulative rules. Searle emphasises the distinction in connection with speech acts (Searle, 1969, p. 33-42). To anticipate a more detailed discussion below, (see chapter 4) in uttering something a speaker performs a speech act. If I say "I promise I'll come tomorrow" then over and above saying it, through saying it I bring something about: I place myself under an obligation. Searle wants to analyse such an utterance by pointing to the things which need to be the case for it to be a satisfactory promise. For it to be a promise certain conditions need to be satisfied. Searle wants to call such conditions rules and to say that fulfilling such rules makes up, or constitutes, promising. He calls such rules constitutive rules.

Rules in this sense are to be contrasted with the more familiar sense of rules in the sense of do's and dont's that constitute rule books. Searle calls these regulative rules.

2.2.2 Controversy about rules

The principal controversy about rules which I want to identify is the idea that when something is appealed to as rule governed there exists a fixed rule which is brought into play. Clearly this is what happens when, literally, during a game a disagreement arises about play and the rules of the game are consulted. However, reflection on for example, the legal process shows how difficult it would be to legislate on every possible eventuality.

Inspired by the position in the philosophy of mathematics from which it takes its name, some sociologists refer to the limited nature of rules as "finitism":

> ... proper usage is developed step by step, in processes involving successions of on-the-spot judgements. Every instance of use, or of proper use of a concept must in the last analysis be accounted for separately, by reference to specific, local, contingent determinants. (Barnes, 1982, p. 30)

Taylor and Cameron (1987) offer a sustained attack on the idea that we can appeal to rules in explaining the structure of conversation. But they do not appear to have grasped the finitist sense of rules which is adopted by, for example, Garfinkel. A thoroughgoing Garfinkelian position on rules would be to hold that rules only enter into an ethnomethodological study in so far as members invoke them in making sense of their world. Unlike Harre and Secord, rules are not assumed to underlie what members do. The professional stance of the student of ethnomethodology is a scepticism about how the world is experienced as orderly and events as reportable.

In offering an example of the confusion over the idea of rule following Taylor and Cameron do little to help matters with their choice of Goffman as an example.

> Goffman says enthusiastically of Garfinkel, for instance, that he
>
> ...extended the argument by going on to look for rules which, when followed, allow us to generate a world of a given kind. To

uncover the informing, constitutive rules of everyday life would be to perform the sociologist's alchemy (Goffman, 1974, p.5).

On one level, what a confusion like this demonstrates is the confusion and eclecticism in social scientists' responses: 'constitutive rules' are a concept familiar from speech act theory, rule following is a Wittgensteinian concept, foreign to generative linguistics. But there is a clear general approval for some kind of rule-based model, and that approval frequently finds support from the specific example of generative grammar. (4)

It is not at all clear to me that Goffman has speech act theory in mind when he talks of constitutive rules. Although Searle uses this idea and although Cameron and Taylor may be familiar with the concept through this source, I do not see that speech act theory has a monopoly on the idea. Secondly whilst I agree that the Wittgensteinian concept of rule following is foreign to generative linguistics I do not see that this remark has any relevance in connection with the bit quoted from Goffman. Is the warrant for this that Goffman talks of rules which "generate a world of a given kind"? Thirdly, the quoted passage continues to the effect that the sociologists alchemy would be the transputation of everyday events into publications. Far from showing clear approval, this seems to me to indicate scepticism.

Rules for turn taking - a paradox?

Given this scepticism about rules why is it then that the seminal paper in conversation analysis, which Sharrock and Anderson (1986, p.61) say many students of the discipline see as the most polished area of ethnomethodological work, is geared to describing rules for turn taking? The answer is, presumably, that Sacks and his colleagues did not set out with an a priori committment to there being rules, rather that procedures turn out to be visible in conversation, thus the rules are discovered in conduct, and are not projected onto it by the analyst. To be sure, Sacks went into the analysis of conversation talking in terms of discovering the "mechanism of conversation", but this does not imply that he saw the project as committed to assuming that a rule based model underlies what speakers do.

3.0 Studying meaning - the "micro-sociologies"

3.1 Symbolic interactionism

The term is due to Blumer (1969, p. 1) although the basic ideas originate with Mead. In an essay entitled "Society as symbolic interaction" Blumer claims that none of the members of the discipline, nor Mead himself, gave a systematic account of the methodological consequences of symbolic interaction for the study of what he calls "group life". In this paper the main themes of symbolic interactionism, interaction mediated by symbols and Mead's analysis of the self, are arranged to deliver a critical account of "the conventional procedures of sociologists". In Blumer's expression of it, the application of symbolic interaction to research entails a kind of ethnography:

> For it means the need for an enriching of experience which will make it possible for observers to form more dependable judgment in those observations which give us our trouble. I don't think that there is any short-cut way of arriving at the formation of such judgements; it has to be done in the slow and tedious manner of developing a rich and intimate familiarity with the kind of conduct that is being studied and in employing whatever relevant imagination observers may fortunately possess.

3.2 Ethogenics

This is the name given to a research project which holds that human social behaviour is the result of rules known by people and which aims at discovering such rules by examining people's explanations, or accounts, for their actions. The idea has been pursued by the U.K. based philosopher of science Rom Harre and has its most complete, and original, exposition in the book mentioned above cowritten with the American social psychologist, Paul Secord (Harre and Secord, 1972). The aim of this book is to claim that an analysis of accounts is the business of a scientifically secure approach to psychology. The book is part of a movement which aimed to usher in, in its own terms, a "new paradigm" in the study of social behaviour. The old paradigm being positivistic, behaviouristic psychology. The idea is ingenious and seems to me to be as follows:

Winch (1958) argues that no social science is possible on the model of the physical sciences for whereas the business of these sciences is to produce laws based on observation of the causal phenomena giving rise to physical goings on no such analysis is possible in the realm of studying people for, following Wittgenstein, human behaviour is rule governed. However, against Winch, describing those very rules is to give a scientifically adequate account of the behaviour under examination. Furthermore, since the rules in question are known by people we can ask them why they did it, and we thereby have a scientific explanation.

The term "ethogenics" itself refers to the generation of behaviour. The unit of the ethogenist's analysis is the rule and the methodology is the analysis of participant's accounts. A large part of the endeavour of The explanation of social behaviour is in attempting to secure the position that Harre and Secord want to adopt. The ideas behind ethogenics, rather than how the research is to be done is the business of the book. A classic study aiming to follow the ethogenic approach was carried out by Marsh, Rosser and Harre (1978) on football violence. The aim of the study is, of course, to describe the rule governed nature of fans' activities. They argue, against the "the moral outrage" which describes fans' behaviour as anarchic, like animals or savages, that it is actually highly organised. They do this by researching the constitutive rules of the fans' behaviour.

In discussing this study Potter and Wetherell (1987, chap. 3) point to what they call "damaging inconsistencies" in the way discourse is handled. They point out that fans' accounts are treated as genuine but that newspaper reports are treated as rhetoric. The point is that what we actually have here are two competing accounts.

Potter and Wetherell point out that ethogenics is unable to discriminate real rules from bogus ones, it cannot deal with the inconsistencies which occur in accounts. They go on to show that the variability in accounts reveals something of great importance - rules are negotiated. The business of the discourse analyst, Potter and Wetherell suggest, is to focus on this very process.

What this discussion masks is the principled objection to rule following as an explanatory resource. My problem is with the very idea that this can be possible. Potter and Wetherell do not address the cogency of the rule idea, for them it is meaningful to talk of rules, but then their data are very different: They are not in the business of inferring rules from accounts because the accounts actually contain rule descriptions.

What Potter and Wetherell go on to look at are the very inconsistencies in scientists discourse <u>about</u> rules. A point that they don't comment on is that in this discourse the accounts are rule-accounts: the scientists are debating the procedures which they see as being constitutive of science, to give one of the fragments that they provide (p. 66):

> [A] means of testing your explanation - [ ] of course that is the absolute, cardinal feature of scientific work, scientific explanations. And that's not just pointing to something that you say is important. It has the following critical value: it allows us to make progress; we can discard theories which have proved useless.

So in this criticism the rule-idea itself goes unchallenged. Rules here emerged in what I earlier called the Garfinkelian position: the mandate for talking about rules is that members do.

I consider the ethogenic rule-based account to be fundamentally defective. Although Wittgenstein does introduce the idea of rule following in his account for reasons that I have given he offers nothing to suggest that people can articulate the rules that he considers them to be following. Indeed it is clear that he thinks that this is not possible, see my discussion of finitism.

The works of Garfinkel (see "ethnomethodology" below) and Gumperz (see "ethnography of speaking") contain imminent criticisms of this appeal to the place of the everyday capacity to offer accounts.

> The member of the society uses background expectancies as a scheme of interpretation. With their use actual appearances are for him recognizable and intelligible as the appearances-of-familiar-events. Demonstrably he is responsive to this background, while at the same time he is at a loss to tell us specifically of

what the expectancies consist. When we ask him about them he has little or nothing to say. (Garfinkel, 1967, p. 37).

If asked, participants could probably supply evaluations such as are cited in sociolinguistic surveys, but this does not mean that speakers rely of such verbalizations in conversation. As we have pointed out, member's situated interpretations take the forms of judgement of intent. All such interpretations presuppose shared social knowledge yet this knowledge is not usually overtly verbalized. Rather it serves as the input for judgements of what the speakers want to achieve. What at the level of survey analysis appear as distributional fact here take the form of typified characteristics of signalling process. It is the fact that it implicitly relies on the everyday knowledge which is acquired through common tradition and shared communicative experience that makes it of interest for the study of social symbolism. (Gumperz, 1982a, p. 36)

(On the theme of survey data versus member's situated reasoning compare with the discussion of Cicourel below.)

Ethogenics starts from an unwarranted assumption about how people behave, an assumption about which there must be considerable doubt.

3.3 Ethnoscience

Sturtevant uses "ethnoscience" as a name for a new approach to ethnography. He tells us that the "ethno" prefix here is not meant to imply that ethnoscience is another branch of ethnography: "The prefix is to be understood here in a special sense: it refers to the system of knowledge and cognition typical of a given culture." (p. 99) Of course grasping the viewpoint of the people being studied is the traditional goal of ethnography but the proposal that culture can be thought of as a conceptual system is novel, Sturtevant tells us. This, together with an attempt to improve the ethnographic method, are put forward as the distinctive features of ethnoscience.

Sturtevant offers six principles of this method. Firstly, the emic etic distinction, discussed above is invoked. He introduces the idea of looking at classification systems in different cultures, and points out that we can't expect the same distinctions to be drawn in different cultures. Secondly with respect to what he

calls "domains" ethnoscience takes a more radical stance than earlier ethnography. "Domain" refers to some category such as "kinship" or "colour". The assumption that there are universal domains is resisted by ethnoscience. Presumably the point is that one prestructures what is going on in a community if one arrives at it with a priori categories. Thirdly, terminological systems is the issue on which students of ethnoscience have concentrated. He quotes Frake:

> The analysis of a culture's terminological systems will not, of course exhaustively reveal the cognitive world of its members, but it will certainly tap a central portion of it. Culturally significant cognitive features must be communicable between persons in one of the standard symbolic systems of the culture. A major share of these features will undoubtedly be codable in a society's most flexible and productive communication device, its language.

Some of the ideas used are explicitly drawn from linguistics. Under the heading of "paradigms and componential analysis" Sturtevant discusses the idea of "contrast set". "This is a class of mutually exclusive segregates which occur in the same culturally relevant environment (setting, context, substitution frame, surroundings, situation, etc.)." (p. 107)

So, taking examples from English speaking culture, "apple" and "sky" do not form a contrast set, but "apple" and "orange" do. These elements are called "segregates". The term "paradigm" (or "paradigmatic set", the two terms appear to be used interchangeably) is applied to a set of segregates which it is meaningful to lump together, thus "apple" and "orange" can be classified as "fruit". A contrast set is a paradigm, although not all paradigms are contrast sets. Presumably this is because a paradigm might not exhaust all the relevant distinctions.

"Componential analysis" is the study of the ways in which the elements in a paradigm differ. The defining features of the segregates are referred to as "dimensions of contrast" or "criterial attributes". The goal of a componential analysis is then to find out what features differentiate the segregates and to then see what components of these features are present or absent in a given segregate. Sturtevant points out that any classification system can be treated in this way. What is distinctive about an ethnoscientific application is that the

paradigm under analysis is a contrast set and that the analysis reflects discriminations meaningful to the members of the community being studied.

Fifthly, another classification of interest is the taxonomy. A taxonomy is one way in which segregates may be related to each other. A taxonomy is conceived of here as a hierarchical structure with segregates appearing at one level only. Sturtevant remarks that a given level of a taxonomy is formed from a contrast set (presumably in the case of a branching hierarchy a contrast set would make up one level of a given branch) and that this may be subjected to componential analysis.

How are these idea to be applied? Sturtevant discusses discovery procedures as his sixth principle. He remarks:

> If an ethnography is to reflect the cognitive system of the bearers of a culture, the validity of the description depends on the discovery procedures. Hypotheses must be checked in the field situation, and revised if they turn out not to fit the data.

Thus analysis of previously recorded data is avoided. An appeal is made instead to procedures of questioning. The model envisaged is very much that of the idea of working with an informant so conversation is appealed to as a medium of communication with the community but also as data itself in so far as the ethnoscientist watches what goes on in spontaneous speech episodes.

Examples of ethnoscientific work are presented on pronominal and case paradigms, kinship terminologies, colour terminologies as well as taste, zoology and disease. Briefly, I want to consider Frake's study of disease diagnosis amongst the Subanun (Frake 1961), to illustrate the ideas behind ethnoscience. The Subanun have been studied extensively by Frake, and, because of his contributions to linguistic anthropology, they frequently occur in this literature. Frake's study of disease diagnosis took place amongst the Subanun of Mindanoa, an island in the southern Philippines. Frake's initial interest was in the social structure of this community, on which I make some remarks below, under "ethnography of speaking". Apparently, to take part in day to day conversations Frake had to investigate terminology relating to folk botany and folk medicine. Amongst his Subanun acquaintances Frake tells us that disease, after litigation

and botany, is the most common topic of conversation. (p. 114) No one has the formal status of being a diagnostician. The Subanun patient "...solicits the readily proffered advice of kin, neighbors, friends, specialists, deities, and ethnographers." (p. 114)

After a discussion of disease names, Frake turns to the issue of diagnostic criteria. "Given a set of contrasting disease names, the problem remains of determining the rules which govern the assigning of one name rather than another in a particular diagnostic situation." (p. 122) He canvassed three ways of deriving rules: analytic, perceptual or explicit. By analytic he means the investigator's establishment of a set of necessary and sufficient conditions to decided whether or not a new instance is a member of a certain category. Such criteria are called "distinctive features".

> The investigator classifies his data into types of his own formulation, then compares "types" as though they were instances of a concept. From information already coded in the definitions of his "types" he derives the necessary and sufficient conditions of class membership.

Frake rejects this procedure since he was not equipped to use the putative etic units of western medicine and in any case not enough illnesses occurred during his period of study for this to be practical. Furthermore, we cannot assume a correspondence between the categories of the two systems, a point made by Sturtevant. For example, even a Subanun child will distinguish *buni* from *buyayag*, two fungal skin infections which, as far as Frake knows, western medicine does not distinguish. (p. 114)

Looking at the perceptual discriminations drawn by the Subanun is also rejected because 1, some disease entities, such as 'headache' are not observable and 2, stimulus attributes, though the overriding considerations, are not the only relevant features for making a diagnosis - situational factors such as the "current social or ecological role demands on the patient" may be important. Presumably, the perceptual investigative procedure, like the analytic one, is impractical being limited by the number of diseases happening in a given time.

Frake points to the limited exposure to different diseases amongst the Subanun themselves and contrasts native learning of plant names with disease categories:

> ...since no one individual ever personally experiences but a fraction of the total number of diseases he can, in fact, differentiate, the Subanun themselves must learn to diagnose disease through verbal description of their significant attributes.

Frake therefore proposes to collect answers to the questions which the Subanun themselves ask when diagnosing disease. This is done by: 1, asking informants to describe particular diseases; 2, asking why diseases were classified in one way and not in another; 3, by following diagnostic discussions and 4; by having a go at diagnosis and receiving corrections. (p. 125)

Frake offers a classification of diagnostic criteria based on the kinds of questions that elicit them:

Pathogenic criteria produced in response to questions about the 'pathogen' (*meksamet*) producing or exacerbating the disease. Prodromal criteria are relevant for enquiries about a prior, distinct condition called a 'prodrome' (*puunan en*). The response to such a question is either another disease name or the declaration that the present disease has no prodrome, that is it is spontaneous. Symptomatic criteria follow enquiries about attributes of the disease. Apparently, judgements of severity can be influenced by obligations which the patient may be under. Etiological criteria are to be distinguished from pathogenic criteria in that the questions that are relevant to ask concern why the disease happened to <u>that</u> person.

Despite frequent reference to the place of: diagnosis; talk about disease; and disease itself in the community, Frake's presentation of diagnostic criteria focuses on an abstract conceptual system. His account hints that the Subanun themselves treat their knowledge in this way. For example, if one of them cannot name a disease, this will be attributed to ignorance, not to there being no name. Thus the realm of disease is exhaustively classified.

3.4 Cognitive sociology

Cicourel's comments on Frake's procedure give a clear view of the direction which he advocates:

> The outputs of the contrastive analysis described by Frake are levels of terminological contrast, and the resulting tables are cross-tabulated outcomes obtained by running diagnostic questions (in the particular examples discussed here) against a range of contrasting answers. Neither the elicitation strategies nor the terminology contrasts, however, specify procedures used by members of the culture or society. (Cicourel, 1973, p.68)

We are dealing, of course, with the ethnographer's methods, not with the methods used by the Subanun themselves.

"Cognitive sociology" can hardly be viewed as an approach in the sense in which ethogenics and ethnoscience represent themselves as new methods. There are close affinities between ethnomethodology and the work discussed by Cicourel in his book, particularly the central place given to member's methods. In fact, Turner (1974) includes a section from Cicourel's study of juvenile justice (see below) in a reader on ethnomethodology and Garfinkel includes Cicourel in the preface to Studies in Ethnomethodology where he says "Over the last ten years a group of increasing size has been doing ethnomethodological studies as day to day concerns..." (Garfinkel, 1967, p. viii). In his own preface Cicourel writes:

> For the past eight years my interests have included a deep preoccupation with how language and meaning are constitutive of the way in which everyday social interaction is assembled and represented.

He goes on to explain the sense in which such issues are antagonistic to methodological collectivism.

> Rather than use notions like role theory, I have tried to substitute terms like 'interactional competence' to indicate a broader idea that would help pinpoint the relations between cognitive processes, contextual emergence, and accounting vocabularies. Social structure remains an accountable illusion of the sociologists common sense knowledge unless we can reveal a connection between the cognitive processes that contribute to the emergence of contextual activities, and the normative accounting schemes we use for claiming knowledge as laymen and researchers.

The approach is eclectic, drawing on phenomenology, ethnomethodology, ethnoscience, ethnography of speaking, "cognitive anthropology" and even cognitive psychology.

Cicourel appeals to the idea of rules, but he differs markedly from the position adopted by Harre and Secord. He sees rules as having a negotiated quality, worked out in the course of an interaction.

> Discussion of normative rules in sociology also tend to be divorced from the interactional settings in which meanings are attributed to objects and events. Research instruments like questionnaires invariably pose hypothetical events or conditions for subjects in the same way as linguists propose sentences as candidates for grammaticality under a system of rules; both situations are divorced from the negotiated interaction scenes in which social organization is produced. The social conditions or sentences studied are given an ideal-type character because they are stripped of the features or particulars and interpretative procedures members use to make general (normative, syntactic) rules creatively relevant to concrete settings. The production of concrete social settings is an on-going accomplishment of the participants. (p. 81)

To get a better glimpse of how Cicourel's ideas inform research I want to turn briefly to his study of The social organisation of juvenile justice. First appearing in 1968 this classic study is based on several years field work in two Californian cities. Through getting into close contact with the people concerned with juvenile crime Cicourel was able to study the day-to-day workings of schools, courts, police and probation departments. In fact he worked for a while as an unpaid probation officer. The thesis is, of course, that an account of delinquency should pay attention to the concrete day to day processes involved in dealing with juvenile crime, which, on this account are seen as constituting "juvenile crime" as a recognized social problem. The first empirical chapter of the book takes statistics rates and types of offenses and asks how these materials (on which conventional sociological research rests) are put together. On the basis of talking to people involved in categorising the data Cicourel concludes that:

> The entire set of procedures for coding police and probation records constituted a continuous improvised set of decisions, whose primary purpose was to achieve practical solutions to problems whose outcomes or resolutions could not be decided according to explicit criteria based upon an explicit theoretical position vis-a-vis the intended meaning of the data. (p. 107)

He goes on to say that methods such as codings and the like are not irrelevant but that what they miss out is the "actor's conception of the operative social structures and the observer's description of the actual scenes".

Subsequent chapters focus on how practical, routine methods produce decisions from concrete situations, on how social organization is depicted in conversations, on routine practices of law enforcement agencies, and the negotiation of dispositions in court hearings.

Language is then fundamental to Cicourel's project of studying how people find sense and structure in their worlds and in his criticism of conventional sociology. His method of analysis, at least in the social organisation on juvenile justice, is interpretative. Apparently the possibility of tape recording conversations was severely limited and so meant that field notes were an important resource.

3.5 The ethnography of speaking

Ethnography refers to the anthropological method of research of going along and observing a group of people over quite a length of time, and getting to know why the participants in that group do what they do. The keeping of field notes and the development of an understanding are the key issues. What has been called the "ethnography of speaking" is then the ethnographer taking a special interest in the talk of groups and situations observed, or the analysis of speech episodes through the application of, or an interest in, an understanding of how speech fulfils social functions. Leading names in the field are Dell Hymes and John Gumperz. The term itself appears to be due to Hymes. He used the expression "ethnography of communication" in a paper appearing in print in 1962. He appears to use the terms interchangeably. Clearly not all communication is spoken (for example sign language of native Australians) and arguably not all speech is communicative. Gumperz also uses the term "linguistic anthropology". Hymes construes the approach as an area within sociolinguistics (1977, p. 83). Note that the title of the book is Foundations in sociolinguistics: an ethnographic approach and that of Gumperz and Hymes (1972) is Directions in sociolinguistics: the ethnography of communication. This book incidentally includes exemplary papers in ethnoscience (Frake) and ethnomethodology (Garfinkel, Sacks, Schegloff). I will say more about the ideas that have come out of this approach to studying communicative behaviour in social contexts in

chapter four. Here I want to sketch what the approach is about and what role attention to language has in it.

In his programmatic papers Hymes emphasises the need for an autonomous discipline oriented to looking at speech practices in communities:

> In short, "ethnography of communication" implies two characteristics that an adequate approach to the problems of language which engage anthropologists must have. Firstly, such an approach cannot simply take separate results from linguistics, psychology, sociology, ethnology, as given, and seek to correlate them, however partially useful such work is. It must call attention to the need for fresh kinds of data, to the need to investigate directly the use of language in contexts of situation so as to discern patterns proper to speech activity, patterns which escape separate studies of grammar, of personality, of religion, of kinship and the like, each abstracting from the patterning of speech activity as such into some other frame of reference. Secondly, such an approach cannot take linguistic form, a given code, or speech itself, as frame of reverence. It must take as context a community, investigating its communicative habits as whole, so that any given use of channel and code takes its place as but part of the resources upon which the members of the community draw. (1964, p. 2-3)

I want to illustrate this approach with two examples, one on a specific kind of event occurring in the Subanun of the Philippines, the other Norway.

In his paper "How to ask for a drink in Subanun" Frake (1964) opens with the suggestion that ethnography would specify what a stranger would need to know in order to participate appropriately in "any scene staged by the society". He uses the case of drinking *gasi*, a rice-yeast fermented beverage which he glosses as "beer", to illustrate that taking part in *gasi* drinking involves knowing more than how to construct a grammatical sentence. In fact he remarks that some speech-play situations require nonsense to be spoken!

Frake's brief paper describes what happens at the festive occasions where *gasi* drinking happens. He says that these drinking situations divide into three "encounter stages" and four "discourse stages", the middle encounter stage having two discourse stages. In each discourse stage Frake tells us that we can talk of the speech acts which occur as having a specific focus and that each stage has a determinate function. The first encounter stage is tasting of the brew. The

corresponding discourse stage Frake calls "invitation-permission" where the focus of speech acts is the expression of roles. Here relations of authority are assigned to the participants. The holder of the jar designates someone to take the next drink. The nominee asks permission to drink of the others and in doing makes a choice from a range of possible forms of address. As the jar goes round the group drinking turns become fewer and longer with certain people dropping out of the round. The encounter stage moves onto "competitive drinking" where the participants pay attention to how much is consumed on each turn. Talk shifts from permission to talk about the quality of the beer. The amount of drinking and talking allocated to a drinker depends on the amount and quality of the verbal responses elicited from the others. On the basis of this encouragement people remain or drop out, until "the encounter is reduced to less that half-a-dozen persons, who can thereby intensify their interaction with each other and with the beer straw." (p.130). Discussion shifts to topics outside the drinking situation, often to litigation and arbitration. In these cases issues are decided on the basis of cogency of argument. Frake remarks that in this society with no "juro-political offices" not only the litigants but those wishing to assume legal authority have to debate effectively.

In cases where drinking continues long enough display of verbal art becomes the focus of speech acts. Songs and verses are improvised and unresolved litigation may continue in this way. Game drinking may accompany this verbal artistry, "Together they help assure that the festivity will end with good feelings among all participants, a goal which is explicitly stated by the Subanan." (p. 131)

Blom and Gumperz (1972) report a study of an important linguistic phenomenon: code switching. According to Gumperz (1982, p. 59),

> Conversational code switching can be defined as the juxtaposition within the same speech exchange of passages of speech belonging to two different grammatical systems or subsystems.

(Compare discussion of frames and schema in chapter 7)

Blom and Gumperz (1972) report that in Norway two languages are officially recognised, Nynorsk and Bokmål, which is spoken in the north. They went to a community in northern Norway where in addition to Bokmål one of Norway's

northern dialects, Ranamål, is spoken. Their paper uses data from three sources: a, formal use of standard elicitation procedures, that is where informants are interviewed and tape recorded, b, unstructured ethnographic study of the community and c, the main part of the paper, recordings of group discussions. During these discussions use of both codes was observed. In fact when sections of the recordings were played back to other residents of the community they thought that the speakers were from some other part of Norway, and when they recognised their voices, expressed disapproval. Some participants showed a similar reaction and offered not to switch in future sessions. Despite this, Blom and Gumperz report that in future sessions, the speaker would tend to use the standard forms when required to "validate his status as an intellectual".

Blom and Gumperz hold that language and society cannot be taken as independent phenomena that can be correlated, "Behavioural regularities are no longer to be regarded as reflections of independently social norms; on the contrary, these norms are themselves seen as communicative behaviour." (p. 432) On the basis of their observations they argue that setting, social situation and social event are ways of trying to explain the speakers' understanding of their behavioural environment "in terms of an ordered set of constraints which operate to transform alternative lines of behaviour into particular social meanings." (p. 433)

These two examples illustrate, in different ways the importance of aspects of discourse in speech situations. I shall discuss this further, and draw on some terms coined in this approach in chapter four.

3.6 Ethnomethodology

Ethnomethodology is an orientation originating within sociology but which, particularly through its approach to studying language, has had a profound impact on other disciplines. In chapter four I will discuss ethnomethodological work on conversation: conversation analysis. Ethnomethodology goes back to Harold Garfinkel and his reaction to orthodox sociology, particularly Talcott

Parson's structural functionalism. The relationship between ethnomethodology and mainstream sociology has often been antagonistic with representatives of ethnomethodology claiming to have no axe to grind about mainstream approaches, but nevertheless implicity claiming that their approach is epistemologically prior. (It would, in fact be interesting to carry out a textual analysis of such issues as the maintenance of disciplinary boundaries in these exchanges). The term comes about as a reflection on tape recordings of jurors' deliberations in a study that Garfinkel was taking part in.

> I was interested in the jurors' use of some kind of knowledge of the way in which the organized affairs of the society operated - knowledge that they drew on easily, that they required of each other. (in Turner, 1974, p. 15)

Inspired by the anthropological terms such as "ethnobotany" to describe folk knowledge of plants, Garfinkel coined the term "ethnomethodology" to refer

> to the availability to a member of common-sense knowledge of his society as common-sense knowledge of the 'whatever'. (p. 16)

It seems to me to be an error to say, as some authors have, that Garfinkel sees the knowledge in question to be social knowledge and that his subjects were lay sociologists. The idea of people producing reportable accounts in the course of practical dealings with one another, if you like, producing, or managing, reality certainly involves social know-how but it is not restricted to it. Thus ethnomethodology was the subject matter of Garfinkel's enquiries but the term generally now means doing sociology in a certain way. Of course, reflexively, Garfinkel's studying of ethnomethodology, of the practices of the members of society, is itself ethnomethodology for what could give it a special status? (I Take up this issue, and the charge of absolutism versus relativism in chapter eight.) I am very much in favour of preserving "ethnomethodology" as the student's name for the folk way of doing things since this keeps in mind that the student's interest is in how people make sense of their world.

An important theme in Garfinkel work is the point that action is accountable. This links up the idea of members moving in an intersubjective world which is usually perceived as normal and understandable and within and against which new events are understood and appraised. Heritage refers to the way in which actions are treated by members as normative or morally accountable as the

"morality of cognition". The following story, reported by Liu Wu-Chi (1955, p. 172),[1] though in another guise, brings out the idea of the routine knowledge of moral accountability.

> To prove the existence of such an intuitive faculty, scholars of Master Yang-ming's school were fond of telling the following oft-repeated story of an unusual encounter between a burglar and a pedant. It seems that the latter, having caught a burglar one sultry summer night, tries to reform him by appealing to his intuitive knowledge of what is good and evil. 'I'm sure,'says the scholar, 'your intuitive goodness will tell you not to commit further trespasses.' But, instead of being convinced the burglar laughs and says mockingly, 'Please tell me sir, where is my good conscience?' At that, the weather being extremely oppressive, the kindly scholar asks his exited visitor to take off his jacket. But still the heat seems to be too much for him. So the host suggests, 'Why not take off your pants too?' To this the burglar protests vigorously, 'That won't be quite proper!' Thereupon the scholar shouts triumphantly, 'Ah! Here is your intuitive goodness!'

How is ethnomethodology to be investigated? Garfinkel offers several ways into explicating the "seen but unnoticed" background of daily life. Chapter two of the Studies reports an assignment of ten tasks set for students where norms were discovered by querying or inverting what are usually taken for granted. For example, students were asked to behave as if they were lodgers in their own homes for one hour, or were asked to haggle over fixed-price goods. Chapter three discusses the "documentary method". This is a process which people carry out but which is not restricted to informal reasoning. It is the incorporation of events into a story which in turn conditions the interpretation of events. Garfinkel makes a theme of its occurrence in sociological research but not as a research topic.

> The method consists of treating an actual appearance as "the document of," as "pointing to," as "standing on behalf of" a presupposed underlying pattern. Not only is the underlying pattern derived from its individual documentary evidences, but the individual documentary evidences, in their turn, are interpreted on the basis of "what is known" about the underlying pattern. Each is used to elaborate the other. (p. 78)

In order to "catch the work of 'fact production' in flight" Garfinkel devised an experiment. Ten undergraduates, told that they were taking part in an evaluation of a psychotherapy technique, talked via an intercom system with a

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[1]This was brought to my notice by Simon Bradley

therapist, in fact an experimenter. The students were told to describe the background to a problem and then ask a yes/no question. After the "therapists" response, which was a random "my answer is yes" or "my answer is no" subjects were told to disconnect the line out to the counsellor whilst they made remarks on the answer. The entire exchange was tape recorded. Garfinkel reproduces transcripts of two exchanges and goes on to list a string of findings. The fundamental point, it seems to me, is that the students heard the counsellor's answers as answers to their questions, answers which, despite being random, the students had no trouble incorporating into their account.

Another chapter of the book is a case study of a young person requesting a sex-change. Garfinkel sees "Agnes" as a practical expert in everyday social affairs, since, despite her biological sex she has learned how to "pass" as a woman.

It will be noted that language is the medium through which the objects of these studies occur, though clearly physical appearance is a domain of which Agnes has explicitly gained command. In chapter four I will continue discussion of ethnomethodology through a consideration of conversation analysis.

3.7 Concluding observations on "micro-sociology"

In ethogenics we found a position with a clearly articulated rationale for its approach and for the research it recommends. I showed that there are conceptual problems with the formulation of human behaviour as rule-governed and particularly with the idea that people consciously know the rules, and that an analyst can grasp them. Symbolic interactionism does not propose a methodology, but instead argues for a kind of non-participant observation which it is able to ground in terms of its own account of understanding. Anthropological approaches to ethnography considered here were ethnoscience and the ethnography of speaking. These both take an interestingly divergent approach to language. For ethnoscience the goal is a description of the community's conceptual apparatus and language is the medium for its discovery. A problem with this approach is that despite its strict avowal of an "emic" approach it

seems to me to project a categorial framework onto a community's knowledge. Furthermore we know enough from the ethnography of speaking to be aware that what people say changes in different instances and as Cicourel pointed out, the result of Frake's ethnoscientific study of Subanun diagnosis does not study members methods. The ethnography of speaking does, I think introduce some valuable formulations to express what it is about and demonstrates the important and integral nature of aspects of speech for a community's conduct. I interpret ethnomethodology as the most thorough going attempt to take seriously the idea that people put reality together and that this process is researchable. I do however have some reservations about some of the claims made by students of ethnomethodology, but this does not affect my interest in their empirical work or the motivation behind it. See chapter four for a more detailed account of ethnomethodological work on language and for further discussion of the ethnography of speaking. I develop my criticism of ethnomethodology in chapter eight.

4.0 Social psychological approaches to practical understanding

In looking at how people make sense of their worlds we have, of course been dealing with an area that falls within the interest of social psychology, indeed Harre and Secord present their book as a text in social psychology. These authors, certainly Secord, would be classified by most psychologists as social psychologists. Of course Mead described his work as social psychology, although he is probably more familiar as a figure in sociology courses. Some authors talk of sociological social psychology and psychological social psychology. Interestingly, a prominent place for consideration of language as a fundamentally social phenomenon in modern psychology are the writings of Vygotsky, in particular a collection of pieces subsequently published as Thought and language, but originally published posthumously in 1934. In the same year Mead's Mind, self and society also appeared posthumously.

A second point of contact with the approaches reviewed above is the fact that contemporary social psychology has roots in phenomenology. Psychology has a

curious relationship to phenomenology. In an article on phenomenology Gilbert Ryle remarked "So 'Phenomenology' only means, as it stands, the science of the manifestations of consciousness and might have been used - though is not - as another name for psychology." If I am not mistaken phenomenology has had its deepest impact on social psychology via the Gestalt psychologists in particular through Lewin's approach and through Heider. Indeed a dominant approach to social psychology, attribution theory, has its origins in Heider's work.

4.1 attribution theory

Heider's suggestion that psychology should take as its subject matter lay psychology has proved to have considerable popularity in social psychology and has adapted well the fashionable project of advancing a psychology of the social world which links up with the prestigious development of cognitive science.

At its heart attribution theory deals with the causes that people hold for why things happen, indeed it is not really a theory at all but a corpus of literature exploring causality in various ways. Heider's suggestion is of course a vague one and I hope the earlier discussion of micro-sociology reveals some of the issues which confront an attempt to study everyday sense-making procedures, mundane reasoning, lay psychology or however whatever it is that people do is to be glossed.

Despite his respect for everyday psychological language Heider holds that it is defective as a scientific account of human psychology. Here attribution theory would part company with most of the approaches discussed above. The most dramatic difference, however, is in its research methods. Attribution theory has been widely explored through laboratory studies. The mandate for this presupposes that the assigning of causes involves fundamental principles which can be teased out experimentally. Such a perspective side-steps studying the construction of explanations as a naturally occurring phenomenon. Although Heider's ideas have been developed in different ways getting at how explaining is done as a routine concern has not been the focus of attention.

For Stratton et al (1986, 1988) attribution theory is simply a set of ways for describing dimensions of causes which have demonstrated their empirical interest. I discuss the method that they advance in the next chapter, where I offer a description of it, and examine the idea of "attributional style" which has enjoyed currency in the application of ideas from attribution theory to clinincal psychology.

This project aims to capture features of explanation in terms of categories brought to the data by the analyst which have an independent interest for the analyst. The emphasis here is not on the construction of explanations.

5.0 Conclusion

In this chapter I have looked at some of the diverse ways in which students of the social sciences have approached language as a researchable topic. I have shown the difficulty of adopting the idea of rules in order to explain social phenomena and how research takes a moral stance when it project models of conduct onto people. In this thesis I shall be concerned with two specific situations, family therapy sessions and interactions in a computer advisory centre, I shall also draw on data from some other sources connected with research on families. I want to start with a study of causal beliefs which, in contrast to the majority of earlier social psychological work on causal beliefs looks at causal statements as they occur in naturally occurring discourse.

CHAPTER 2: DESCRIBING CAUSES AND THE IDEA OF EXPLANATORY STYLE

This chapter uses a method of discourse analysis to illuminate a controversy in social and clinical psychology. The method is the Leeds Attributional Coding System (Stratton et al. 1986, 1988) developed by my colleagues at the Leeds Family Therapy and Research Centre. The controversy is whether or not people tend to offer explanations which show a certain kind of pattern, or "attributional style". The kind of explanations under consideration are causal statements.

After an introduction to the concept of attributional style I present a detailed account of its relationship to the idea of personality traits and go on to show the contribution that a study of causal beliefs in naturally occurring discourse can make to the idea that people have styles in the way that they attribute cause. I argue that the study of causal beliefs has been arbitrarily constrained by the use of questionnaire methods. I develop a general model of attribution style which yields a set of hypotheses that I test on data taken from family therapy sessions and interviews with parents. I find that certain patterns do indeed occur and that a given person can show more than one pattern. A portion of the data enables me to test whether or not a style is shown across different situations; I do not find any evidence for this.

1.0 Attributional style

"I failed the exam because I'm just stupid. "
"I failed the exam because I had an 'off' day. "

It seems intuitively clear that the explanation that a person believes for why an unpleasant outcome happened will affect the way that they react to that outcome. This intuition has been developed by social and clinical psychologists using a certain model of explanation giving and a behavioural model of responding to undesirable events. The seminal paper in this project, Abramson,

Seligman and Teasdale (1978), sought to rectify problems encountered by Seligman's learned helplessness hypothesis (Seligman 1975). "Learned helplessness" refers to the tendency for an organism to fail to make avoidance behaviour when in an unpleasant situation as a result of prior exposure to that situation where no improvement in the situation followed any behaviour emitted by the organism. The claim is that an organism can be trained to be helpless. The learned helplessness hypothesis holds that states such as depression can be explained through learned helplessness. Now not only might we feel uneasy about the cruel animal experiments on which the learned helplessness phenomenon was established, we might wonder how depression can be thought of in this way. For example can the general helplessness which a depressed person feels result from exposure to specific situations of noncontingency? Abramson et al (1978) respond to two problems for the learned helplessness hypothesis of depression. Firstly the hypothesis does not distinguish cases where a person faced with an aversive stimulus comes to believe that: 1, no solution can remove the stimulus, that is that anyone in that situation would be helpless; and 2, a solution is possible but that they lack the ability to provide it. Secondly the hypothesis could not explain differences in the severity of depression and why a person exposed to noncontingent situations should generalise their learning to be helpless to so many situations.

The solution to these problems was sought in attribution theory, which I introduced in chapter one. This is an approach in social psychology to everyday explanations stemming from Heider's work, for example Heider (1958), and developed by Jones and Davis (1965), Kelley (1967), and Weiner (1972, 1974). Attribution theory focuses on the explanations which people hold for why things happen, that is the causes to which they attribute outcomes. It is clear why such an orientation was attractive to defenders of the learned helplessness hypothesis. Attribution theory provides a conceptual apparatus to describe the explanations that a person offers for why their actions do not ameliorate an unpleasant state of affairs. So with respect to the first inadequacy of the hypothesis we can distinguish internal and external attributions for noncontingency. With respect to the second inadequacy we can use the idea of a person's explanations to define attributions for an unpleasant state of affairs which are global or local

(specific) or which are stable or unstable.

Given that some people, but not others, become depressed in the face of unpleasant situations it is not surprising that researchers have sought an explanation in terms of people's beliefs about the causes of the situations that they find themselves in and what they can do about it. Furthermore it is not surprising that the attributional reformulation model has been applied in describing and researching people's explanations. It is proposed that individual differences probably exist in attributional style. Those people who typically tend to attribute failure to global, stable, and internal factors should be most prone to general and chronic helplessness depressions with low self-esteem. By the reformulated hypothesis such a style predisposes depression. Does such a style of attributing causes actually exist?

This confluence of clinical and social psychological perspectives has lead to a great amount of research on the relationship between depression and the kinds of things that a person holds to be causes of negative things in their life. Although there is a lot of evidence that certain measures of explanation giving are associated with depression the literature is contradictory and whether or not there is a causal relationship between explanation and vulnerability to depression has received detailed attention (Brewin 1985). Robins has recently argued (Robins 1988) that studies failing to find a relationship between explanations and depression are statistically flawed. He claims that the tests used in rejecting the null hypothesis of no relationship are not powerful enough.

Whatever the explanation for the null results the studies reviewed by Robins are remarkably uniform in one respect: they all use the same method for studying and conceptualising people's explanations - The Attributional Style Questionnaire (ASQ), due to Peterson, Semmel, von Baeyer, Abramson, Metalsky and Seligman (1982), or a variant on it.

1.1 The Attributional Style Questionnaire

The ASQ instructs subjects to provide explanations for twelve hypothetical

situations and then asks them to rate their choice of answer along the attributional dimensions, stable/unstable, global/specific and internal/external. In addition a judgement on the importance of the situation is also asked for. The ratings are achieved by offering definitions which subjects are asked to apply by ringing a number between one and seven. The situations are intended to represent two kinds of "goal area" achievement and affiliation, and two kinds of outcome, good and bad. For example (from p.291):

"You become very rich" (good achievement)
"You meet a friend who compliments you on your appearance" (good affiliation)
"You can't get all the work done that others expect of you. " (bad achievement)
"You go out on a date and it goes badly. " (bad affiliation).

To illustrate this and to show the definitions given here is an example from the questionnaire.

YOU MEET A FRIEND WHO COMPLIMENTS YOU ON YOUR APPEARANCE.

1) Write down the one major cause _____

2) Is the cause of your friend's compliment due to something about you or something about the other person or circumstances? (Circle one number)

Totally due to the other person or circumstances	1	2	3	4	5	6	7	Totally due to me

3) In the future when you are with your friends, will this cause again be present? (Circle one number)

Will never again be present	1	2	3	4	5	6	7	Will always be present

4) Is the cause something that just affects interacting with friends or does it also influence other areas of your life? (Circle one number)

Influences just this particular situation	1	2	3	4	5	6	7	Influences all situations in my life

5) How important would this situation be if it happened to you? (Circle one number)

Not at all important	1	2	3	4	5	6	7	Extremely important

Granting that causal explanations can be thought of in terms of the dimensions of attribution theory there seem to me to be two major objections to the ASQ. Firstly, the questionnaire offers outcomes and elicits explanations. The situations portrayed could be meaningless for the person filling in the questionnaire. For example some of the situations which it presents, such as "You get a raise" may simply be inappropriate for some groups of people. Stratton and Swaffer (1988) produce evidence that non-student subjects consistently misinterpret the instructions. How can we be sure that the kinds of explanations offered in a pen and paper situation represent the kinds of explanations that a person feels to be true in a real situation? Perhaps they don't, maybe there is an arbitrary but consistent relationship between ASQ scores and symptoms of depression. If this is the case then the ASQ can teach us nothing about the development, maintenance and communication of explanatory tendencies. Secondly the ASQ yields information on only a very small number of the causal beliefs held by the person to whom it is given. If we are to be able to talk of consistencies and variations in an individual's ways of attributing causes we need many more beliefs than the twelve cases studied by the ASQ. Thirdly, the ASQ does not study a person's beliefs about why things happen to another person. Fourthly it does not study the person's beliefs about the controllability of the outcome in the situation, a dimension stressed by Weiner (1979)

Might there be other ways to analyse causal beliefs? There have been various attempts to analyses spontaneous causal beliefs. In his critical review of the literature, Munton, (1985-86), refers to two attempts to establish the criterion validity of the ASQ by correlating ASQ with written accounts. The work was not published and was inconclusive. Peterson, Bettes & Seligman (1982). A second similar study was also carried out (Castellon, Ollove & Seligman 1982) this again was unpublished. In the paper on the ASQ we are told that

> In several studies, we have shown that ASQ scores correlate positively with actual attributions made by subjects for specific events, such as rejection in a dating situation, poor performance at laboratory tasks, and the occurrence of stressful life events.

> When "naturally" occurring attributions are extracted from therapy transcripts and rated blindly along our three dimensions, high correlations with the therapist's ratings of depression are observed. (p. 297)

We are not shown, however, the contexts in which their subjects made "specific attributions" nor how these statements are converted into ratings which yield data capable of statistical analysis. So the validity of the ASQ remains doubtful.

Munton has published results concerning an instrument which was developed at the LFTRC, the Leeds Attributional Coding System, (LACS).

1.2 The Leeds Attributional Coding System

The LACS aims to code, that is to categorise and thereby offer a concise description of, causal statements in discourse. It appears to be the most detailed method yet developed for analysing causal beliefs in natural discourse. It consists of a set of definitions, developed in order to establish intersubjective agreement of analysers, of each attributional dimension. In chapter three I look at what features of expressions lead them to be considered as "attributions" or expressions of causal beliefs. I discuss in more detail there some of the issues in defining an "attribution". Here I just want to remark that in the LACS a causal belief is seen as a belief about a causal chain consisting of a cause, a link, and an outcome. Now each of the attributional dimensions could be applied to each stage of such a sequence. For example in a statement such as: "I got promoted because I'm lucky." We could look at the stability of being promoted, (outcome) luck as a cause of promotion (link) or being lucky (cause). In practice the LACS codes only one of this triad, see definitions below. In addition it redefines some of the dimensions, for example clarifying the relationship between the internal/external and the personal/universal dimension. It proposes five dimensions which are binary pairs: stable/unstable; global/-specific; internal/external; personal/universal and controllable/uncontrollable. These are defined in chapter three of the LACS which explains the rationale for the choice of these dimensions and where the following definitions are elaborated on. I have extracted the following definitions by way of illustration:

1. Stable/Unstable
If the cause is believed by the speaker to be more likely than not to apply in the topic of the outcome in the future, then **stable**; if it would only apply about half of the time or less, then **unstable.**

2. Global/Specific
If the cause is believed to be likely to influence at least a moderate range of outcomes which are regarded as non-trivial by the speaker, then **global**; if few such links, then **specific.**

3. Internal/External
If the cause is believed to originate in the person being coded, then **internal.** If it originates outside the person, whether a characteristic or behaviour of another person, or a circumstance, the **external.**

4. Personal/Universal
If anything in the attribution (cause, line, or outcome) is believed by the speaker to indicate something particular about that person, then code idiosyncratic or **personal.** If it would apply to any normal member of the appropriate reference group (as defined by the speaker) then it is **universal.**

5. Controllable/Uncontrollable
If the speaker believes that the person being coded could normally manage to significantly influence the outcome in the absence of exceptional effort to circumstance, then **controllable.** If the causal sequence is believed to be inexorable or the outcome inevitable in normal circumstances, then **uncontrollable.**

For example (Stratton et al. 1988,p. 84)

Dad: Because Jean is different from me she gets aggravated.

Is classified as stable, global, external, universal and uncontrollable.

The ASQ does not clearly distinguish events where the subject is the agent and events where the subject is the target. When looking at real causal beliefs we find a variety. The LACS records who the speaker is, and in the case of an event caused by a person, who that person is. (This person is termed the "agent".) [1] In addition, in the case of an event which happens to someone, who that person is. (This person is called the "target"). The dimensions of internal, personal and controllable are coded separately for speaker, agent and target.

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[1]Note that this term does not imply agency in the sense discussed in chapter three below.

So in the above example the target is Jean, and in addition to the coding reported for Dad (the speaker), for Jean the last three dimensions are coded internal, personal and uncontrollable. In the study which I report below I shall, however, restrict myself to codings for the speaker alone to keep the calculations manageable. Often there I will adopt the practice of labelling an attributional classification by translating the binary coding into a denary number, again for manageability. Thus a classification of "stable, global, internal, personal and controllable" would be indicated by "11111".

In addition to coding for these five attributional dimensions the LACS also describes coding for aspects of the outcome namely its desirability, significance and whether or not it is hypothetical or actual. Since I will need to consider negative outcomes specifically in the studies reported below, here is the definition used (Stratton et al. 1988):

> Positive/Neutral/Negative Outcome
> If the outcome is felt by the speaker to be desirable or **positive** then [2]. If it is undesirable or **negative**, then [0]. If **neutral** then [1]. If undecidable but not neutral, score [3].

Speakers may produce many utterances which are codable attributions, for example in a one hour family therapy session the principal speaker may produce about forty attributions. The result of applying the LACS is therefore a table of many such codings.

Clearly the LACS would seem to have great potential in making researchable aspects of the giving of causal explanations which the ASQ does not investigate. My strategy is this: If individuals do have a tendency to believe explanations of a certain kind then this should show up in the LACS codings. By looking at the descriptions of explanations yielded by the LACS we have an empirical test of whether or not attributional style exists. The idea is that if someone has a an above chance proportion of a certain pattern then there are grounds for talking of them showing an attributional style.

Freeing ourselves from the constraints imposed by the ASQ, many variations of what attributional style might mean become possible.

1.3 Models of attributional style

The idea that some people have attributional styles is fundamental to the application of attribution theoretic ideas in clinical psychology. The explanatory power of the learned helplessness theory of depression rests on the idea that depressed people in particular have a specific style for explaining negative events.

> Individual differences probably exist in attributional style. Those people who typically tend to attribute failure to global, stable and internal factors should be most prone to chronic helplessness depressions with low self esteem . . . Our model predicts that attributional style will produce depression proneness, perhaps the depressive personality. (Abramson et al. (1978, p. 68)

The notion of attributional style due to Seligman and colleagues, that people who are taken to be depressed tend to offer causes which are stable, global and internal when explaining a negative outcome, emerges as just one model. What other models might there be? The basic idea is clear. We are in the business of talking about patterns in the causes offered to account for events. But is it patterns produced by each person for any kind of event, or patterns produced for a certain kind of event by any person? Does the pattern depend on the mood of the person, on the situation where the causal belief is expressed? Clearly models of differing strengths can be imagined. Other researchers might add restrictions concerning the mood of the person being rated to the model, others might underplay the style issue as a property of the individual, emphasising the situation as the factor. Fundamentally we are talking about a regularity or pattern in our data about explanations. A pattern could mean many things (for example a tendency to blame a certain child for family upsets). What we mean here of course is a pattern in terms of the attributional dimensions and the further categories made available by the LACS.

Rather than survey the literature for diverse notions of the style concept an alternative strategy is possible. By including the aspects which can vary in the proposal of a model of attributional style we can construct a schema which will systematically generate any possible model of attributional style. Anyone proposing the existence of attributional style is making a statement about causes

which quantifies over people, outcomes, topics (issue being discussed), and context (the situation in which the expression occurs). For example Seligman holds that causes which are global, stable and uncontrollable, tend to be offered by people which are "depressed", or are prone to depression, when explaining negative outcomes. Is this restricted to major negative outcomes? Compare loss of a spouse with loss of a paperclip.

So we can construct the following sentence frame:

When talking about {all/certain} topics in {all/certain} contexts {all/certain} people produce causal explanations for {all/some} of {all/certain} kinds of outcomes, these are {all of the same type/predominantly of one type/from a certain range of types}.

Note that here "type" exhausts the scope "style" in the sense of the ASQ. I hope that it is now clear how the ASQ restricts the range of phenomena that may be relevant. Concentrating on type alone for the moment (the last set of brackets) the "depressive attributional style"; stable, global, internal (for significant negative outcomes) is just one type. We can consider types involving all the attributional dimensions, or any combination of these. If we have five dimensions, stability; globality; internality (for speaker); universality (for speaker) and controllability (for speaker) then there are 31 possible combinations of these as follows:

5	combinations	takings	dimensions	one at a time	S,
10	"	"		two "	SG,
10	"	"		three "	SGI,
5	"	"		four "	SGIP,
1	combination	"		five "	SGIPC.

In general for n dimensions the number of combinations are given by the nth row of Pascal's triangle (less one since we do not consider taking no dimensions); or ${}^{n}C_{r}-1 = n!/r!(n-r)!-1$.

Now for each of these types, the value on each dimension is free to vary. For example, considering the combination of SG we have four possibilities namely

stable & global; unstable & global; stable & specific and unstable & specific. I propose to call each of these a model. With the LACS each dimension has one of two values, so in general we have 2^n models, where n is the number of dimensions in the combination.

So in the case of five dimensions, for example, we will then have $5\times2+10\times4+10\times8+5\times16+32=$ 242 types!

Before looking at the data I want to look more closely at the concept of attributional style and the background from which it emerges.

2.0 Conceptual analysis of "attributional style"

Cutrona, Russell and Dallas Jones (1984) raise the question: do people have a style of making attributions which endures across different situations? They construe "attributional style" as a personality trait, and say that the strength of the concept should be assessed in terms of the controversy about personality traits. I think that a detour through a portion of the debate on personality traits to which they refer would be desirable. The justification is firstly, to explore what the claim that an individual has a specific and enduring way of making attributions amounts to and secondly, to examine what the consequences are for attribution theories and theories dependent on them if they do not. The debate in question is between trait theorists who argue that people have fixed characteristics across different situations and others holding that the specific situation is more important. The debate implicitly raises some fundamental issues.

1, The role of the intuitive models of personality which individuals hold and how these differ in their desiderata from theories in psychology. 2, The methodologies used by psychologists in investigating how individuals come to make causal and personality judgements.

These are substantive issues but whilst the appropriateness of statistical techniques has received explicit attention in this debate (see Epstein, 1979, 1980)

little attention has been given to the conditions surrounding the formulation of personality judgements in the laboratory. (For example the so-called demand characteristics of the judgement task). All proponents in the debate seem to be aware of this to some extent but it does not seem to emerge as an issue of importance.

The controversy referred to is to be found in a pair of series of exchanges 1, From Mischel (1968) to Epstein (1979, 1980) and 2, the debate in Psychological Review, Mischel and Peake (1982) with replies by Funder, Bem and by Epstein (1983), counter reply (1983) and comment by Cowley (1984).

Epstein (1979) offers a summary of the debate and gives arguments for and against - not only situationist and trait theories but interactional theories as well. Epstein's approach is interesting since he advocates a methodological resolution to the debate (indeed the second paper of the pair is a polemic on the appropriateness of statistical methods in psychology at large). I have taken as a starting point for the debate Mischel's (1968) book - but one could start with Cattell, Murray - or Theophrastus.

In his book Personality and Assessment (1968), Mischel states that: 1, How someone acts depends to a large degree on the situation that they are in. 2, Trait theories reify and nominalise processes which we refer to in everyday psychological talk. He sees this tendency as dehumanising and asks:

> What would happen if we treated the organism as truly active and dynamic rather than as the carrier of a stable dispositional character of motives and traits? (1968)

The relationship of psychological theories of personality to commonsense "theories" will be a recurring theme in this chapter. The source of Mischel's notoriety amongst personality researchers undoubtedly lies in his hurtful suggestion that the coefficient of 0.2 to 0.3, which is apparently invariably found when a personality dimension inferred from a questionnaire is correlated with a criterion from a different medium, should be called the "personality coefficient". Looking at this suggestion in its context, chapter 14 of Personality and Assessment, it is clear that the butt of Mischel's argument are personality

questionnaires. Nevertheless, Mischel's work, however restricted in its criticism, is generally perceived as the stimulus for subsequent papers.

Alker (1972) replies on a variety of lines which don't cohere closely: 1, "situational specificity in responses is itself a personality variable" (see also Bem and Allen (1974)). 2, Different statistical designs (factor analysis) would pick out interactions between person and situation variables. 3, A Psychodynamic model can accommodate the required flexibility. That is, a more dynamic model of what a trait is would rescue the concept from the rigidity which Mischel criticises.

Bowers (1973) replies, not merely to Mischel, but to situationism as a creed. He groups together positivism and Skinnerian behaviourism as antecedents of situationism. He advises us that it is a necessary and warranted corrective to trait psychology, but he sees both perspectives as flawed; and he uses his critique of situationism (namely that it rejects the part played by the "organismic and intrapsychic determinants of behaviour") to put forward his own resolution: "interactionism". (For what is meant by this see the section on Epstein (1979) below).

During 1973 "organismic and intrapsychic" factors appear in Mischel's thinking as he unveils a "cognitive social learning theory of personality". Here Mischel gives full reign to the role of individual differences by incorporating, or seeking to incorporate, thought as a partly situation-free component into his model.

The issues at stake in using a nomothetic methodology to investigate what is after all ideographic looms large in Bem and Allen's (1974) paper. This paper is especially interesting because it is here that the clearest equation of trait theory with commonsense occurs. One or the other must be wrong, they say, research or intuition. I shall argue that this is a ridiculous disjunction; but one that pervades both sides of this debate. Epstein (1979) writes:

> A critical issue in personality theory is whether stable behavioural dispositions, or traits exist. On the basis of everyday observation,

> it seems to most people that they do. Yet the vast bulk of psychological research fails to provide confirmatory evidence. It must be concluded that either the lay view is right and that the typical methods of research are lacking, or the research findings are correct and the lay view itself is a phenomenon worthy of study.

Aren't the beliefs of lay people a core part of the psychologist's subject matter anyway?

Mischel and Peake (1982) tackle three papers which they construe as putative resolutions to the trait - situation debate. 1, Epstein (1979): "multiple behaviour aggregation", 2, Bem and Funder (1978): "template matching", 3, Bem and Allen (1974): "moderator variable approach" (some-of-the-people-some-of-the-time). Each of these is examined; and in the case of 1 and 2 studies are reported which failed to replicate the proponent's findings. The conclusions are:
1, Epstein's method establishes reliability but bypasses the problem of situational consistency. 2, The methods of Bem and Funder and Bem and Allen are highly restricted in their application.

Epstein (1983) replies that his goal was precisely to examine the effects of aggregation on reliability and validity; not to confront the cross-situational consistency issue. He raises an important point:

> Their [Mischel and Peake] view maintains a source of confusion that has characterised the person situation debate throughout its long history: namely that cross-situational consistency can properly be investigated by examining the inter-item correlation of a sample of items selected casually by face validity. (p 179)

As a solution to this Epstein advocates a dose of "whole-test correlation factor analysis" (p. 183) to select items. So here Epstein's tack is to argue that he is not immersed in the debate, that behaviour is both general and specific. The problem is, according to him, how behaviour is to be dissected into items. This is a crucial point. I shall argue that this solution, whilst true, is nevertheless bankrupt.

Bem and Funder each reply separately. Bem adopts a methodological theme, claiming that the template-matching study which failed to replicate his work with Funder focuses on a by-product of the method, not on the method itself. In

reference to his study with Allen, Bem adopts the remarkable step of claiming that some of the orientations adopted were merely whimsical. Funder's reply is important because the first two points that he makes distinguish his paper from the others. The third of his three points concerns the role of aggregation in the prediction of behaviour. The two novel points are: 1, The assessment of situations, and very importantly 2, The basis of lay impressions of personality.

The response from Mischel and Peake isolates agreement amongst these papers and argues that a clarification of method would be beneficial. They parallel the debate in personality theory with the debate in psychotherapy. We should ask not "Does Psychotherapy work?" but rather "When does such-and-such a type of psychotherapy work?". So we see that the resolution advocated is a selective refinement of areas of disagreement. No reference is made to the role of lay personality beliefs. This tendency is cemented by Cowley's "Comment on the Mischel-Epstein debate" which looks exclusively at the issue of temporal stability or cross-situational consistency issue.

The bankruptcy which I refer to in Epstein's resolution is that he is seeking to resolve a problem which is too fundamental to be rectified by a methodological manoeuvre. We would expect that a judicious enough selection of hypothetical components of behaviour can be made, such that those components are to be found in behaviour in different situations. That is simply a statistical problem, which despite Epstein's murmurings about replacing intuitive behavioural categories with statistical ones, still has at its base the awkward insertion of lay beliefs into a statistical framework. I would say that the functioning of lay personality beliefs has its own logic which cannot be assumed to obey the laws developed by statistical theory.

The situation - personality trait debate seems to ask the question "what is personality, how can it be measured, how can it be studied scientifically?" what could instead be asked is: "What do people's personality beliefs enable them to do? " or "how does the rhetoric of everyday psychology work?"

I think that when Cutrona et al. (1984) write that:

> A new trait concept has been added to our already voluminous dictionary of personality characteristics. Although this proposed trait is not likely to be incorporated soon into common parlance...

they unwittingly touch on what for me is the key issue here. Real trait terms as used by people in everyday situation are part of a system of rhetoric through which experience is structured.

A trait, however, is not the only model of how an aspect of personality appears to be an enduring feature of someone. In fact it is not to "trait" but to "cognitive style" that Peterson et al look in the development of the ASQ. In connection with a discussion of the stability of attributional scores they claim that the ASQ results are "respectably high" and relate the ASQ scores to other measures of "cognitive style", making reference to the review by Goldstein and Blackman (1978). The issues are however the same. Attribution theorists have not paid detailed attention to causal expressions in their natural habitat.

3.0 Studies of attributional style

Application of the LACS.to transcripts of conversation should yield data on whether of not styles occur. In this section I describe three studies. Study one draws on data from family therapy sessions; studies two and three add data from a colleague's study of interviews with parents and assessment interviews with families.

The method used to carry out the analyses is rather detailed. I propose to describe this first.

Application of the LACS to this material creates a few thousand records, each of which is a 11 binary number, relating to the five attributional dimensions with internal, personal and controllable repeated for agent and target. In addition the identity of speaker, agent and target is also included plus codings for the significance and negativity of the outcome. The task is to analyse these to find whether a pattern is present.

How can the data be analysed to test the attributional style concept? This divides into two problems, a theoretical one and a statistical one:

1, What predictions does attributional style make? What hypotheses does this yield in terms of the data available? 2, What statistical technique is required to test such hypotheses?

Recall the sentence frame developed above:

When talking about {all/certain} topics in {all/certain} contexts {all/certain} people produce causal explanations for {all/some} of {all/certain} kinds of outcomes, these are {all of the same type/predominantly of one type/from a certain range of types}.

Cycling through all the combinations supported by this schema we can generate 96 models of what attributional style might mean. Of course some of them will probably have no adherents. Taking the first option in each bracket would yield an implausibly strong position. How many can be tested by the data available?

1, The data provide no measure of the topic.
2, The data are all drawn from therapy sessions, context in the sense of social situation cannot be tested. Nevertheless context in the sense of the pragmatics of an utterance does of course vary in therapy sessions, from pleas to injunctions, however the data provide no measure of this. (I shall argue in chapter four that there are principled objections to the possibility of such an analysis.)
3, The restriction to negative outcome can be tested. As can the uncontrollable negative outcome favoured by some researchers.
4, We have no grounds to pre-classify the people involved. So only the model pertaining to "certain" people could be tested here. The limited numbers and range of subjects means that this model cannot be tested in a definitive way here.

5, The data also allow us to test hypotheses relating to whether or not the speaker is the agent or target. So we can look for the occurrence of pattern amongst the dimensions of attributions coded for each speaker in three ways. Considering all outcomes, negative outcomes and uncontrollable negative outcomes.

A bifurcation also operates here. We can separate cases where the speaker is the target (the outcome happens to them) and cases where the target is someone else. This yields six hypotheses. The dependent variable will be a measure of pattern. Here I shall only study attributions where the speaker is the agent or target. The discrepancy between a speaker's attributions for self and others emerges as a key theme in my colleague Jo Silvester's doctoral work on parent's and carer's beliefs about their children. The model I shall test is therefore restricted in a number of ways.

Across how many dimensions might a pattern hold? Should a tendency to offer a high proportion of stables, a high proportion of globals and a high proportion of internals be considered as a pattern or should just the composite stable, global internal?

I discussed above that these choices can be considered as "types", giving a more general model of what an attributional style might be in terms of attributional dimensions. How can I tell whether or not style is present?

3.0.1 Criteria for detecting styles

One way would be to apply a log-linear analysis to the data. This procedure generates models of interaction across the dimensions, this corresponds to a multidimensional version of a chi-square treatment to find associations between two variables. Munton (1987) uses log-lin but with some reservations about the independence assumption.

Another problem for our data is that we must expect many empty cells in the multidimensional contingency table. What does this mean? The literature discusses the computational problem of expected frequencies of zero and the

case of predicted zeros or "structural zeros", for example the expected frequency of the category of "pregnant men". I have found no discussion of whether or not log-linear modelling can be applied to contingency tables which are simply "sparse". Considering limitations known to apply to chi-square we could reason as follows: For a chi-square analysis to be applicable the expected frequency per cell should be at least five. Now if we have five dimensions, each having two values, then we have a 2^5, 32, cell contingency table. If the chi-square criterion applies we would need at least $5 \times 32 = 160$ scores (at least because these would need to be uniformly distributed). In the case of individual subjects our data does not have as many scores as that, so by this criterion, log-linear modelling is not reliable.

It seems wise to proceed with a simpler form of analysis which makes fewer assumptions. We can at least find out the number of each models that occur.

Looking at the kind of outcomes that occur is a computational problem to which there may be different solutions. Construing each model as a variable, which will be a binary number having as many digits as there are dimensions in the model, we can obtain a frequency count of those outcomes that occur. Each outcome may be a style component, if certain outcomes occur beyond a chance level then we will have ground for talking about an attributional style. Defining the variable itself is a tedious problem since all the combinations need to be generated. [2] In practice I shall only consider dimensions three at a time and five at a time. Such an analysis could be run for different style hypotheses:
For different type combinations we could consider all attributions or just those where the speaker is the target or where the speaker is the agent and at the same time we could consider these three possibilities with different outcome conditions: all outcomes, or negative outcomes only.

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[2] I am grateful to Nicos Drakos of the computing department, University of Leeds for writing a programme to do this step.

What could we conclude from such an analysis? A measure of the likelihood of a certain number of a certain outcome can be reached as follows.[3] Say we have n observations and find r of a certain outcome. In general, for n independent trials of some event with probability p, the probability of that event occurring r times is described by a binomial distribution. The probability of such an output is given by the expression:

$$Pr(p,r,n) = {}^{n}C_{r}(p)^{r}(q)^{(n-r)}$$

Where (q = (1-p)), the probability of the outcome not occurring

For large n and p approximately equal to q the binomial distribution approximates to the normal. Unfortunately this is not the case here: although n is large the distribution is highly skewed since p is not close to 0.5. If the probability of either of the two outcomes for each dimension is 1/2 then for two dimensions it is 1/4 for three dimensions 1/8. Nevertheless using the above equation values of Pr(p,r,n) can be constructed for a range of values of r. Of course consulting tables is not feasible because of the large number of enquiries that need to be made. Consider a study of style at the level of five dimensions and involving ten subjects, each having a different number of attributions. For each subject I would need to produce thirty two tables (since p takes a different value for each model). I would therefore need to make, and consult 320 tables. I developed a computing routine which both calculates critical values of r and decides whether or not the frequencies observed are significant. The values reported which I report below were calculated by a program the core of which was written by Paul Nicholson (See appendix 1). The program selects numbers of different combinations of the attributional dimensions which occur outside the frequency which would be expected by chance. I chose a 1% significance level and performed a two tailed test since the combinations could occur at a frequency of greater than or less than would be expected by chance. In other words, I took combinations which had less than a one in a hundred chance of occurring to be cases of attribution style. As I mentioned, the binomial

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[3] I'm indebted to my friend Walter Leser for reminding me of this.

distribution is highly positively skewed. Considering it graphically, each distribution consists of a series of blocks, one for each possible value of r. As I am interested in values of r which have a chance of less than 1% of occurring I need to consider values of r which have a probability of less than 0.5% or greater than 99.5%. Selecting values of r during which the area beneath the distribution reaches 0.995 is not adequate, the critical value of r needs to be wholly in the critical region. The program therefore selects values of r such that the cumulative probability of r-1 is greater that 0.995. Below are a series of studies where I applied this criterion in order to study 1, whether or not people show a style of attributing causes on different occassions, 2, whether or not people generally show such styles at all and 3, what kinds of styles are shown. The first three studies consider the five dimension of stable, global, internal, personal and controllable developed in the LACS; the next three focus on the dimensions presented in the learned helplessness theory of depression, stable, global and internal for negative, uncontrollable outcomes.

3.1 Studies of attributional style at the level of five dimensions

3.1.1 Study 1: Are there cross-situational attributional styles? (i)

This study aims to test the hypothesis that an attributional style is held across different situations. The data for the study were transcripts of family therapy sessions selected from the LFTRC archives. I chose transcripts from the middle of therapy, sessions four and five, when change in the subjects' beliefs would not be expected to occur. In the case of family 3 I studied the transcript from session four and six because due to an error in the arrangements for the session, session five was very short. The hypothesis is, of course, that any subject showing a style in the first session will show the same style in the second one.

Data

The transcripts were analysed using the LACS procedure and yielded 483 codable attributions for twelve people. The families are composed as follows: family 1: subjects 1 and 2, family 2: subjects 3,4,5 and 6, family 3: subjects 7,8,

and 9 and family 4: subjects 10,11 and 12. A sample of 51 codings were cross-coded by an experienced coder as a reliability check and Cohen's kappa calculated for these. See table 2.1.

TABLE 2.1 RELIABILITY OF CODINGS.

Dimension	Kappa	standard error	z	
stable	0.105	0.144	0.732	NS
global	0.297	0.123	2.418	*
internal	0.960	0.039	24.417	*
personal	0.355	0.162	2.185	*
universal	0.431	0.136	3.173	*

* significant, $p < 0.01$

A significant kappa was found for all the dimensions except stability. Using the binomial procedure described earlier, model decisions were made for all the subjects under four cases: 1 taking all attributions, 2, selecting those where the outcome was negative, 3 selecting those where the speaker was the target and 4, selecting those where both the outcome was negative and the speaker was the target.

Results

Certain speakers do show styles in the sense defined above. The following tables show which speakers showed which styles for different kinds of attributions, namely whether the speaker is the target in the attribution and whether or not the outcome is negative. This information is split into two tables for the convenience of layout. In addition the tables show the number of attributions of that type were made and the number of attributions that the speaker made.

Clearly the number of attributions considered may drop as some are excluded in the selection. For example speaker 1 in table 2.2a, therapy session II, had seven attributions which I didn't rate as having a negative outcome, the number dropping from 31 to 24.

TABLE 2.2a STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ALL ATTRIBUTIONS AND ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE.

speaker	attributions considered: all				negative outcome			
	I		II		I		II	
1	-		11110	6/31	-		11110	6/24
2	01110	2/11	-		01110	2/7	-	
3	-		10101	4/17	-		-	
4	10110	3/23	10100	6/16	-		-	
5	11111	3/9	00101	2/11	-		-	
6	11111	2/3	-		-		-	
7	-		-		-		-	
8	-		-		-		-	
9	-		-		-		-	
10	01000	8/48	-		01101	3/28	-	
11	11000	16/38	-		-		-	
12	-		-		-		-	

TABLE 2.2b STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ATTRIBUTIONS WHERE THE SPEAKER IS THE TARGET IN THE ATTRIBUTION AND ATTRIBUTIONS WHERE BOTH THE SPEAKER IS THE TARGET AND THE OUTCOME IS NEGATIVE.

speaker	attributions considered: speaker is target				speaker is target and negative outcome			
	I		II		I		II	
1	-		-		-		-	
2	-		-		-		-	
3	-		10101	4/16	-		-	
4	10110	3/17	-		-		-	
5	11111	3/6	00101	2/5	-		-	
6	11111	2/3	-		-		-	
7	-		-		-		-	
8	-		-		-		-	
9	-		-		-		-	
10	01000	5/23	-		01000	5/15	-	
	01101	4/23			01101	3/15	-	
11	-		-		-		-	
12	-		-		-		-	

In the cases where all attributions are considered, two people show styles in both sessions, namely subjects four and five. But in neither case is it the same style. Any temptation to retrieve a style by "factoring", for example selecting 101-1 from 10110 and 10100 form subject four, should be resisted. The criteria

by which these occurrences were selected as significant involved dimensions other than those factored out here. So the getting stable, specific, internal, personal, uncontrollable and stable, specific, internal, universal, uncontrollable in a test of five dimension does not indicate whether or not stable, specific, internal uncontrollable would be significant in a test of four dimensions. Looking at the other selections, in only one case did a speaker show a style in both sessions. This is subject five again where the target is the speaker. But it is a different style in each sessions.

It is interesting to note that a speaker may show more than one style. Speaker 10 in table 2.2b holds both unstable, global, external, universal, uncontrollable and unstable, global, internal, universal, controllable. The possibility of a person holding two styles might seem odd, against the background of the ASQ, but of course it might depend on just what is being explained: the person could be talking about different classes of events. The ASQ model implies that it only applies to uncontrollable outcomes, but as I mentioned when discussing the ASQ, it does not attempt to get information on the subject's perception of controllability for the outcome described. Because subject ten has two dimensions going in different directions, the difference on one can remove any sense of a contradiction in the change in the other. For example, here the person may be interpreted as saying that whilst many uncontrollable, unstable, global and external events are universal, controllable unstable, global and external events are personal.

In these data there is evidence that speakers do show attributional styles in the sense that they use a causal statement instantiating particular models more than would be expect by chance. Consideration of different kinds of explanation, in the sense of whether the outcome was negative or whether the speaker was the target of the event being explained, make a difference to whether or not a style is revealed. With respect to the specific issue of whether or not speakers have a cross-situation style, no speakers produced the same pattern in the two sessions studied.

3.1.1.1 Styles at the level of families

Munton and Antaki (1988) report that families can show patterns of attributions at the level of a group. By collapsing the scores for each member of the four families that I studied I obtained the following results. Again, the styles that occur are shown under different conditions depending on which attributions were considered. The entries in the table can be compared with table 2.2a and 2.2b to compare the contribution from each speaker. In the case of one family, family four in table 2.3a for all attributions, showed the same style in both sessions. This was 11000 or stable, global, external, universal, uncontrollable.

TABLE 2.3a STYLES SHOWN BY EACH FAMILY IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ALL ATTRIBUTIONS AND ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE.

speaker	attributions considered			
	all		negative outcome	
	I	II	I	II
1	-	11110 9/46	-	11110 6/31
2	11111 9/73	00101 3/48	11111 9/50	00101 3/31
		10101 8/48		
		10101 5/48		
3	-	11111 4/47	-	-
4	01000 12/89	00000 5/65	01000 6/47	-
	11000 34/89	11000 23/65	01101 5/47	-

TABLE 2.3b STYLES SHOWN BY EACH FAMILY IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ALL ATTRIBUTIONS, ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE, WHERE THE SPEAKER IS THE TARGET AND WHERE BOTH THE SPEAKER IS THE TARGET AND THE OUTCOME IS NEGATIVE.

speaker	attibutions considered			
	speaker is target		speaker is target and negative outcome	
	I	II	I	II
1	-	11110 7/31	-	11110 5/22
2	-	-	-	-
3	-	-	-	-
4	01000 9/43	-	01000 5/25	-
	01101 3/43	-	01101 3/25	-

3.1.1.2 Styles present when results from both sessions are pooled

Table 2.4 shows the style which are found when the scores from each subject are pooled together. It is interesting to note that styles found earlier may no

longer be significant comparing these results with table 2.2a and 2.2b. For example, subject 1 showed 11110 as a style in session II when all outcomes were considered; this no longer occurs as a style. In addition, style previously held may change. For example subject 2, in table 2.2a held a style of 01110 (unstable, global, internal, personal, uncontrollable) in session I. In table 2.4, pooling the scores from session I with session II, this subject shows a style of 11110 (stable, global, internal, personal, universal).

TABLE 2.4 STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ALL ATTRIBUTIONS, ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE, WHERE THE SPEAKER IS THE TARGET AND WHERE BOTH THE SPEAKER IS THE TARGET AND THE OUTCOME IS NEGATIVE.

speaker	attributions considered							
	all		negative outcome		speaker = target		negative outcome & speaker = target	
1	-		-		-		-	
2	11110	5/26	-		-		-	
3	11110	10/86	10101	4/43	-		11110	6/30
4	10101	9/36	-		10100	6/27	-	
5	00101	2/20	-		00101	2/11	-	
	11111	3/20						
6	11111	3/7	-		11111	3/6	-	
7	00000	5/57	-		-		-	
8	11000	15/35	00101	2/14	-		00101	2/9
9	-		-		-		-	
10	11000	28/82	01101	3/34	01000	5/40	01000	5/10
11	11000	24/60	-		-		01101	3/20
12	-		-		-		-	

In summary subjects who show a style in one of the sessions separately and in both together are: all attributions: 4,5,6,11; negative attributions: 10; where the speaker is the target: 6 and 10; where the speaker is the target and the outcome is negative: 10. These results are difficult to interpret, in some cases looking at the number of each pattern shows that the patterns leading to a style being significant all occur in just one of the sessions. Certainly the results do not lend any evidence to the hypothesis that people have cross-situational attribution style. Table five shows the results obtained when the families are collapse into an aggregate for both sessions together.

Table five shows the results obtained when the families are collapse into an aggregate for both sessions together.

TABLE 2.5 STYLES SHOWN BY EACH FAMILY IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ALL ATTRIBUTIONS, ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE, WHERE THE SPEAKER IS THE TARGET AND WHERE BOTH THE SPEAKER IS THE TARGET AND THE OUTCOME IS NEGATIVE.

speaker	attibutions considered			
	all	negative outcome	speaker = target	negative outcome & speaker = target
1	-	11110 9/81 11111 6/81	-	-
2	11110 20/178 11111 12/178	11110 14/147 11111 10/147	11110 15/120	11110 11/47
3	00000 6/101 11111 34/101	00000 6/101 11000 34/101	-	00101 2/27
4	00000 8/154 11000 57/154	00000 8/154 11000 57/154	-	01101 3/38

In order to look more closely at how prevalent attributional styles are, and at which styles occur, I carried out two further studies. Since carrying out my computing procedure is itself a long task and since a lot of data is required for detailed testing I made use of coded data already held within the LFTRC.

3.1.2 Study 2: Are there attributional styles at the level of five dimensions?

Aim

This study pursues the finding made above that styles do occur, that there is a range of possible styles and that an individual may show more than one style.

Data

The data for this study come from interviews carried out and analysed by my colleague Jo Silvester in the course of her doctoral work. I am deeply indebted to Jo for sharing these data with me. The interviews are from three sources. One, interviews with parents of children attending a, a local Social Services Nursery and b, the University's nursery run by the Students' Union. This yielded data from nineteen people. Secondly, interviews with mothers of children (between the ages of four and a half to five and a half years) attending an

Infant School in York (fifteen subjects). Thirdly, interviews with thirteen families carried out at the Dept of Psychological Medicine of Great Ormond Street Hospital, London, where they had been referred for assessment (nineteen subjects).

Again, reliability of the codings was tested by having a sample cross-coded by another coder. The results are shown in table 2.6:

TABLE 2.6 RELIABILITY OF CODINGS

Dimension	Kappa	Standard Error	z
stable	0.474	0.119	3.969 *
global	0.425	0.130	3.263 *
internal	0.595	0.122	4.859 *
personal	0.532	0.132	4.016 *
controllable	0.513	0.138	3.719 *

* $p < 0.001$

There is a choice about what starting values we use for the calculation of the binomial probabilities. I have decided to use mean probabilities across the whole data set and have included the data used in study one above, considering scores for each session as separate. This means that some of the data are drawn from the same people. The reason for doing this is that it is the occurrence of a style in an episode of conversation. Pooling the separate sessions together presupposes a consistency across situations. This yields a total sample of fifty three subjects.

Results

I counted the number of patterns found to be significant at the 1% level for each subject for different selections of attributions. The results are summarised in table 2.7.

The occurrence of a style must be counted as common. Considering all outcomes, and negative outcomes, more subjects show a style than do not. We may also note than in these cases a "compound style" is also found. That is, speakers show more than one style.

TABLE 2.7 NUMBER OF PEOPLE SHOWING CERTAIN NUMBERS OF STYLES

	Attributions selected			
Number of styles shown	All	negative outcomes	speaker = target	negative outcome & speaker = target
0	4	22	22	41
1	15	24	21	11
2	21	6	10	1
3	8	1	-	-
4	3	-	-	-
5	1	-	-	-
6	1	-	-	-
Number of people	53	53	53	53
Number of Attributio ns	3689	1694	1703	822

Certain, possible models are not found at all. The following table gives the frequency of those models that occur. (See table 2.8)

The model which occurs most frequently as a style is thus 11000, namely, stable, global, external, universal and uncontrollable. This is the modal style out of all the attributions considered. Pursuing the train of thought behind the reformulated model of learned helplessness this would be a depressive style but for the choice of external rather than internal and universal rather than personal. The triad of stable, global, internal put forward in the that theory of depression does however form a common model (20.5% for all attributions) namely: stable, global, internal, personal, controllable.

TABLE 2.8 THE FREQUENCY WITH WHICH EACH MODEL OCCURS AS A STYLE

Model	Attributions considered			
	All	negative outcome	speaker = target	negative outcome & speaker = target
00000	20	5	4	1
00001	3	2	7	2
00010	1	1	1	-
00011	-	-	1	-
00100	-	-	-	-
00101	1	2	-	-
00110	2	-	1	-
00111	8	3	2	2
01001	2	1	2	1
01001	-	-	-	-
01010	-	-	1	-
01011	-	-	-	-
01100	-	-	-	-
01101	-	-	-	-
01110	-	-	-	-
01111	-	2	-	-
10000	4	-	-	-
10001	-	-	1	-
10010	-	-	-	-
10011	-	-	-	-
10100	-	-	-	-
10101	-	-	-	-
10110	-	-	-	-
10111	10	3	1	1
11000	22	12	5	2
11001	-	-	-	-
11010	-	-	3	-
11011	-	-	-	1
11100	-	-	-	-
11101	2	-	1	-
11110	4	5	2	2
11111	25	4	9	1
	104	40	41	13

3.1.3 Study 3: Are there cross-situational attributional styles? (ii)

These data can also serve as a pool from which to obtain another empirical measure of the probability of the different values of the different attributional dimensions. The following tables report a recalculation of the data used in study one based on the empirical p values of pooling those data with these.

TABLE 2.9a STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ALL ATTRIBUTIONS AND ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE.

speaker	attributions considered							
	all				negative outcome			
	I		II		I		II	
1	11101	6/50	11000	8/31	11001	4/25	11110	6/24
			11110	6/31				
2	-		11000	7/15	-		11000	5/7
3	11000	11/38	10101	4/17	11000	8/5	-	
4	-		10100	6/16	-		-	
5	11111	3/9	-		-		-	
6	11111	2/3	-		-		-	
7	11000	10/38	-		-		-	
8	11000	6/12	11000	9/23	-		-	
9	-		11000	4/5	-		-	
10	01000	8/48	11000	12/34	01101	3/28	-	
	11000	16/48	11100	7/34	11000	9/28		
11	11000	16/38	11000	8/22	11000	7/15	-	
	11101	6/38						
12	-		-		-		-	

This procedure yields more styles altogether including instances which support the cross-situational model. In the case of all attributions five subjects show styles in both sessions and two, speakers eight and ten show the same style in each, namely 11000, stable, global, external, universal, uncontrollable. No speakers show the same style in both sessions when only attributions with negative outcomes are considered but under the selection for attributions where the speaker is the target, the style 11000 becomes significant for speaker eleven. This style remains significant for this speaker when attributions with negative outcomes are imposed on this selection. Whilst I would expect there to be differences in the proportions of the different dimensions coded in the different groups studied here; I cannot eliminate the possibility that the different proportions found, and hence the different style found here, are due to differences in coding practice.

TABLE 2.9b STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) CONSIDERING ATTRIBUTIONS WHERE THE SPEAKER IS THE TARGET IN THE ATTRIBUTION AND ATTRIBUTIONS WHERE BOTH THE SPEAKER IS THE TARGET AND THE OUTCOME IS NEGATIVE.

speaker	attibutions considered							
	speaker is target				speaker is target and negative outcome			
	I		II		I		II	
1	11101	5/31	11000	5/23	-		-	
			11100	5/23				
2	-		11000	3/8	-		-	
3	-		10101	4/16	-		-	
4	-		10100	3/10	-		-	
5	11111	3/6	00101	2/5	-		-	
6	11111	2/3	-		-		-	
7	11000	5/24	-		-		-	
8	-		11000	4/14	-		-	
9	-		-					
10	01000	5/23	11100	6/17	01000	5/15	-	
	01101	4/23	-		01101	3/15		
11	11000	6/23	11000	5/15	11000	5/10	11000	4/8
	11101	6/23						
12	-		-		-		-	

3.2 Studies at the level of three dimensions

As mentioned above, Seligman and colleagues have placed a lot of emphasis on the idea that there exists a style: stable global internal for the attribution of causes with negative uncontrollable outcomes, which they associate with depression, or a tendency towards depression. The presence of this style occurs as a component of the five dimensional study above, but as I mentioned in the case of "factored" scores in study one the occurrence of two styles at the level of five dimensions does not enable conclusions to be drawn about any common dimensions. This study therefore runs the analysis used above, but for just three dimensions. There is, however, an added complication. Stratton et al. (1986) show that the "internal" dimension employed by Seligman taps information from two sources which are separable into an "internal" and a "personal" dimension. For this reason my analysis here will cover both possibilities separately, stable global internal and stable global personal. In addition cases when the speaker is the target will be tested since, in the case of testing a concept relating to

depression explanations where things the occur happen to the speaker are especially relevant. As in study two the data set consists of seventy seven sets of coded attributions.

3.2.1 Study 4: Are there cross-situation attributional styles? (iii)

This study re-examines the data used in study one, selecting only negative and uncontrollable outcomes. Table 2.10a shows the styles found for stable, global and internal dimensions, table 2.10b corresponding results for stable, global and personal dimensions.

TABLE 2.10a STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) FOR THE DIMENSIONS OF STABLE, GLOBAL AND INTERNAL CONSIDERING ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE AND WHERE THE OUTCOME IS NEGATIVE AND THE SPEAKER IS THE TARGET.

speaker	SGI			
	negative outcome		negative outcome & speaker = targ	
	I	II	I	II
1	-	111 10/21	-	-
2	-	-	-	-
3	-	-	-	-
4	-	-	-	-
5	-	-	-	-
6	-	-	-	-
7	-	-	-	-
8	-	-	-	-
9	-	-	-	-
10	-	-	010 5/9	-
11	-	-	-	-
12	-	-	-	-

Very few styles are shown at all and no speaker shows a style in the two sessions, let alone the same style. One speaker, speaker 1, shows the "depressive attributional style" for stable, global and internal attributions, but not for the case where the speaker is the target.

TABLE 2.10b STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) FOR THE DIMENSIONS OF STABLE, GLOBAL AND PERSONAL CONSIDERING ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE AND WHERE THE OUTCOME IS NEGATIVE AND THE SPEAKER IS THE TARGET.

speaker	SGP			
	negative outcome		negative outcome & speaker = target	
	I	II	I	II
1	-	-	-	-
2	-	-	-	-
3	-	-	110 5/7	-
4	-	-	-	-
5	-	-	-	-
6	-	-	-	-
7	-	-	-	-
8	-	-	-	-
9	-	-	-	-
10	-	-	010 5/9	-
11	-	-	110 5/7	-
12	-	-	-	-

3.2.2 Study 5: Are there attributional styles at the level of three dimensions?

Although fewer speakers appear to hold styles when considering three dimensions some do. Table 2.11 shows how many speakers showed styles. Only one speaker showed a compound style.

TABLE 2.11 NUMBER OF PEOPLE SHOWING CERTAIN NUMBERS OF STYLES

	SGI		SGP	
Number of styles shown	negative outcome	negative outcome & speaker = target	negative outcome	negative outcome & speaker = target
0	36	46	50	46
1	17	6	3	7
2	-	1	-	-
3	-	-	-	-
4	-	-	-	-
Number of people	53	53	53	53
Number of Attributions	1379	649	1379	649

Table 2.12 shows which styles occurred. The "depressive style" was not common. The style of stable, global, personal was the most frequently occurring style for negative outcomes, accounting for one quarter of the styles found in that case.

When only negative outcomes where the speaker is the target were considered, however - the paradigm case presented in the ASQ - this pattern ceased to be significant, that is, it disappeared as a style.

TABLE 2.12 SHOWING THE FREQUENCY WITH WHICH EACH MODEL OCCURS AS A STYLE

	SGI		SGP	
Model	negative outcome	negative outcome & speaker = target	negative outcome	negative outcome & speaker = target
000	4	3	2	4
001	2	1	3	-
010	1	-	-	-
011	-	-	-	-
100	-	-	-	1
101	-	-	-	-
110	9	2	3	3
111	1	2	4	-
	17	8	12	8

3.2.3 Study 6: Are there cross-situational attributional styles? (iv)

Finally I have once again pooled my data with Jo Silvester's in order to obtain a broader population of the proportion of the possible codings on the different dimensions. Tables 2.13a and 2.13b show styles occurring in the two therapy sessions considered earlier.

Although few styles are shown at all, in one case a speaker did show a style in both sessions and it is the same style in each, stable, global, external for negative outcomes. Also of interest is that one speaker does show a "depressive style", stable, global, personal for negative outcomes and negative outcomes where the speaker is the target.

TABLE 2.13a STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) FOR THE DIMENSIONS OF STABLE, GLOBAL AND INTERNAL CONSIDERING ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE AND WHERE THE OUTCOME IS NEGATIVE AND THE SPEAKER IS THE TARGET.

speaker	SGI			
	negative outcome		negative outcome & speaker = targ	
	I	II	I	II
1	-	110 10/21	-	111 8/7
2	-	-	-	-
3	110 10/14	-	-	-
4	-	-	-	-
5	-	-	-	-
6	-	-	-	-
7	-	-	-	-
8	-	-	-	-
9	-	-	-	-
10	-	-	-	-
11	-	-	-	-
12	-	-	-	-

TABLE 2.13b STYLES SHOWN BY EACH PERSON IN THERAPY SESSIONS 4 (I) AND 5 OR 6 (II) FOR THE DIMENSIONS OF STABLE, GLOBAL AND PERSONAL CONSIDERING ATTRIBUTIONS WHERE THE OUTCOME IS NEGATIVE AND WHERE THE OUTCOME IS NEGATIVE AND THE SPEAKER IS THE TARGET.

speaker	SGP			
	negative outcome		negative outcome & speaker = target	
	I	II	I	II
1	-	110 11/21	-	-
2	-	110 5/6	-	-
3	110 9/14	-	-	-
4	-	-	-	-
5	-	-	-	-
6	-	-	-	-
7	-	-	-	-
8	-	-	-	-
9	-	-	-	-
10	-	-	010 5/9	-
11	110 8/12	-	-	-
12	-	-	-	-

Finally, I would like to remark that the definition of an attributional style that I have used will count patterns which occur significantly few times as a style. The final version of the computer program used uses the data itself to generate patterns to test, so it will in fact only look at the probabilities of patterns that occur somewhere in the data. This is the case with the present data set, but in no case did a person show any style in this sense at all, so the possibility of holding such a style in both sessions did not arise. Styles in this sense do occur. Considering Jo Silvester's subjects, in the case of negative outcomes, nine people showed a a pattern with significantly few occurrence six people showing unstable, global, external and three stable, specific, external. Considering the dimensions stable, global and personal, for negative outcomes, three people showed unstable, global, universal, two showed stable, specific, universal and one showed stable, global, universal.

3.3 General discussion

The six studies only examine a small range of the possible meanings that could be attached to the concept of attributional style. In particular I have not separately considered cases where the outcome is significant, nor have I looked at issues surrounding who the agent in the attribution is. I addressed the idea that speakers have a cross-situational attribution style in studies 1, 3, 4, and 6. Considering scores from separate people, that is ignoring the analysis carried out on families considered as a group, this provided 168 occasions where the same style could be present in both of two therapy sessions. Each study considered a dozen speakers and each looked at four different considerations. Studies 1 and 3 taking all attributions made, ones with negative outcomes, ones where the speaker is the target and finally one where both the speaker is the target and the outcome is negative. Studies 4 and 6 looked at the triads stable, global, internal and stable, global, personal for attributions with negative outcomes and ones where the speaker is the target and the outcome is negative. I found only five instances where speakers did show the same style in both sessions. Judging the implications of these results statistically is tricky, what I think is clear is that this test counts as evidence against the claim that people in general hold cross-situation attributional styles.

Looking at the results another way, speakers show different styles on different occasions.

A basic pair of findings is that 1, there are many attributional styles, not just the depressive one and that 2, speakers may hold more that one style.

3.3.1 Discussion of LACS method

I think that it is clear that if people's understanding of causes are to be conceptualised in terms of the categories provided by attribution theory then the LACS is an improvement on the ASQ and on attempts to content analyse transcripts. I have, however, reservations about each of the steps involved in the application of the LACS. Firstly, the isolation of causal beliefs itself. Even if a coder can extract casual statement the problem remains that gauging the significance of those beliefs for the person is difficult, in addition there could be contextual constraints on the problems that come up for discussion and on how these are formulated. Secondly, the task of coding an attributions is not easy. In defence of the method the achievement of intersubjectively agreed codings distinguishes it from alternative procedures to carry out this kind of research.

4.0 CONCLUSION

I presented a conceptual analysis of the idea of attributional style, showing that many possible versions of it are possible, particularly when the confines of the ASQ are broken out of. I then outlined a non-questionnaire based method for researching causal beliefs, the LACS. Applying this required the development of a statistical approach and a computation procedure to impliment it.

Within the limitations of the LACS, and I think that the LACS is more valid than a questionnaire could ever be, a study of families in therapy failed to show impressive evidence for the existence of a cross-situational attributional style. I did find that styles, however, do occur, and importantly that there is more than one style that does occur and that an individual can show more than one style. The styles tested formed only a portion of those possible. A thorough test of

the idea that there is a depressive attribution style would of course require a group people who have been diagnosed as depressed. These are not directions which I will move in here. I want to take up the issue of the expression of causal beliefs, leaving aside, attribution and the attempt to describe causes as an analytic strategy.

CHAPTER 3: IDENTIFYING CAUSAL BELIEFS IN NATURAL DISCOURSE

The passage from attempts to study causal beliefs through questionnaires to looking at the expression of causal beliefs in conversation and writing raises the issue of what it is for an utterance or inscription to be an expression of a causal belief. Attribution theorists are divided about what kind of expression counts as an attribution. An "attribution" here means a causal belief. The disagreement thrown up most clearly in the literature is whether or not reasons are distinct from causes, and if so should be counted as attributions. Davidson (1980) argues that reasons are a species of cause. I present his arguments and point out that the distinction between reasons and other causes is important in looking at what speakers achieve by explaining an event in terms of one rather than the other. I conclude that reason explanations a, can be distinguished from non-reason causes and b, should not be excluded from a study of causal beliefs. I report a study of the verbal features of those expressions which analysers have considered to be "attributions" in transcripts of family therapy sessions and interviews. I find that although "because" (or its variant "cos") is the most common sentential connective to occur in the expressions identified as attributions this accounts for less than half of the expressions selected. The remainder are achieved by coordination (juxtaposition of cause and outcome), by other connectives or by linking with a verb having a causal semantic component.

1.0 Explanation and the reason - cause distinction

What is a causal belief? A causal belief is a belief which relates two events, a cause and an effect. The occurrence of the cause is offered as an explanation for why the effect happened. In this chapter I want to look at the variety of expressions which analysers of transcripts of stretches of talk have considered to be causal beliefs. I hope to clarify our grasp of what a causal belief is and to assess the scope of carrying research on causal expressions.

The term "attribution" appears on page 22 of the Leeds Attributional Coding System Manual (LACS). The context makes it clear that this term is used as a synonym for "causal belief". The scope of the term "causal belief" is discussed here, in particular whether or not explanations involving reasons are to be included. I want to look at this in some depth but independently of this issue we should note a different distinction which is implicit in the attribution theory literature.

In ordinary language an attribution need not be directly related to explanation at all causal or otherwise. A property could be attributed to someone or something without a cause in sight. Clare's skill in singing blues could be one of her attributes but a speaker could refer to this without expressing any kind of cause. "What a good singer Clare is!" So Stratton et al. (1988), along with attribution theorists in general, use attribution in a narrow sense. Naturally a person's attributes are likely to enter into causal explanations that a speaker offers. But simple descriptions, even the ascription of dispositional properties, such as impatience or, to give a non-psychological example, fragility, are not counted as attributions, unless they occur as part of an explanation. So "the vase is fragile" would not be counted as an attribution but "the vase broke because it's fragile" would be. We should note that this sense of "attribution" is wider than one that is used by some authors who restrict the term to an explanation for a significant event, as opposed to any happening.

Some writers have felt that attribution theorists have concentrated on causal beliefs to the exclusion of other explanations, such as reasons. Buss (1978) holds that they are confused about the nature of causes and reasons and that this has led to constraints being placed on how attribution theorists have interpreted people's explanations. I think that the authors of the LACS are correct when they say that attribution theory is equally applicable to reasons and causes. They adopt an agnostic position following a reference made by Kidd and Amabile (1981) to some remarks which Braithewaite (1953) makes in opening a discussion of the variety of explanations.

> Any answer to a "why?" question may be said to be an answer of a sort. So the different kinds of explanation can be best appreciated by considering the different answers that are appropriate to the same or different "why?" questions. (p. 319)

Kidd and Amabile go on to point out that this is not, of course, a sufficient definition of an explanation. "How?", "When?" and "Where?" may also receive explanations as answers.

I shall argue that reasons are a species of cause and so I hold that (if attribution theory is applicable to causes then) attribution theory is applicable to reasons. It doesn't follow from this reconciliation that attribution theorists are not confused as Buss suggests. However I don't agree with Buss's sketch of the cause - reason distinction, nor do I agree with the route taken by the authors of the LACS. They write:

> ...Whatever the philosophical view, no clear demarcation between teleological explanation and causal explanation that can deal successfully with specific examples has yet been established. (p. 23)

I think that this is wrong because teleological explanation is clearly distinct from causal explanation, or to use Aristotle's terms an explanation in terms of final causes can clearly be demarcated form one in terms of efficient causes. A teleological explanation cites some end point as the cause. An example would be "Stones fall when dropped because they tend towards the centre of the earth". Introducing gravity switches this to a causal explanation. "Stones fall when dropped because gravity exerts a pull on them". This seems to me to be a clear distinction. The authors doubtless mean "reason explanation" not "teleological explanation", it is wrong to assume that these are interchangeable. Buss consistently identifies reason explanations with teleological explanations. I don't think that such an equation is possible. Citing wants, desires, yearnings and intentions as explanations yields non-teleological explanations. "Simon went to the pub because he wanted a drink". This involves an end point; Simon having his pint, which is an end point because it hasn't occurred when Simon did the setting off which is up for explanation, but I take it that the explanation is in terms of Simon wanting that end-point. That wanting is not an end-point. Braithwaite wrote:

> Teleological explanations of intentional goal directed activities are always reducible to causal explanations with intentions as causes;

> To use the Aristotelian terms, the idea of a 'final cause' functions as an efficient cause; the goal directed behaviour is explained as a goal directed behaviour.

1.1 Reasons as causes

In their discussion of causes and reasons neither Buss nor Kruglanski consider an ancient, and I think natural, position: reasons are a kind of cause. When we cite reasons for someone's action, "Sue went home early because she wanted to avoid the rush" it seems to me that we are offering an explanation for Sue's action, going home early, in terms of her desire to avoid the rush and that this desire is a cause of the action. Doesn't the explanation work by saying "Sue's desire to avoid the rush caused her to go home early"?

The classical defence of freedom of the will in the face of causal determinism hinges on the view that reasons are causes: If all events are caused (causal determinism) then how can someone's doing something be something done freely? It can be done freely if it is caused by the person's beliefs and desires. This view of reasons as causes has been held to be defective by several philosophers recently. It's to this work that Buss and Kruglanski refer. Following views expressed by Wittgenstein in lectures given the 1930s many philosophers of action have held that reasons cannot be causes.

In The Blue and Brown Books Wittgenstein states that:

> Giving a reason for something one did or said means showing a way which leads to the action (p. 14)
>
> The proposition that your action has such and such a cause, is a hypothesis. The hypothesis is a well founded one if one has had a number of experiences which roughly speaking, agree in showing that your action is the regular sequel of certain conditions which we then call the causes of the action. In order to know the reason which you had for making a certain statement, for acting a particular way, etc., no number of agreeing experiences is necessary, and the statement of the reason is not a hypothesis. (p. 15)

In an influential paper the contemporary American philosopher, Donald Davidson (1980), has persuasively argued that reasons are a species of cause

meeting many of the objections that Wittgenstein and others put forward for the distinction. Before representing Davidson's position I want to explain some of the terms that crop up here; event, action, agency. An event is simply something that happens once, the sinking of the Bismarck, the scoring of the winning goal in the 1985 FA cup final, the Wall Street crash, the end of the ice age, the depression the formed over the Atlantic last week. Note that all these events can, in principle be dated. My paying for the milk each week is not an event, but a series of events. Note that events do not have to be, and probably are not, instantaneous, the end of the ice age presumably took several decades. An event may consist of other events. The hundred years' war was an event, and although each of the battles constituting it were events, the battles constituting the hundred years' war are not an event.

Some events are actions. Clearly an event which someone brings about intentionally is an action, Bill stamping on Ben's hat for example. But Ben's hat getting blown of by the wind is not an action. (Unless we attribute intentionality to the wind). Now if Bill stamped on Ben's hat it follows that Bill stamped on a hat. What if Bill stamped on a hat believing it to be Ben's not realising it was his own? Now Bill unintentionally stamped on his own hat, but that unintentional stamping must be an action of Bill's; since Bill stamped on a hat surely is an action. So Davidson concludes that an event is an action where intention is relevant. The author of such an event may be called an agent and we say that the event shows agency.

To meet Wittgenstein's argument we have to show that a cause need not be a hypothetical statement of the kind that he suggests. But surely (qua scientists) we don't want to deny that causal statements relate to laws which are confirmed by their instances (disconfirmed by their exceptions) which enable us to make predictions. What is the relationship between cause, laws and induction?

Hume teaches us that "we may define a cause to be an object, followed by another, and where all objects similar to the first are followed by objects similar to the second". This states that every true singular causal statement entails a strict law governing events of the kinds to which the events belong. This is a

thesis about what we do when we flank "caused" by two events; we propose a necessary or logical connection. If it is true that event f caused event g then it is true that any occurrence of event f will cause event g. As Davidson puts it any singular causal statement is "backed by" a strict law. The problem is of course, that we are (usually) unable to state what that law is. The argument so far has been that any singular causal statement means what it does through claiming to be an instance of a law. The acknowledgement that we don't know what the law enables Davidson to reconciles Hume's view of cause with that of Ducasse:

> The reconciliation depends, of course, on the distinction between knowing there is a law 'covering' two events and knowing what the law is: in my view Ducasse is right that singular statements entail no law; Hume is right that they entail that there is a law. (p. 160)

If Davidson's analysis is correct then it frees us to talk of singular causal statements, such as, "I posted the letter yesterday because I wanted it to reach Rosa in time for her 18th birthday", as being causal statements without being concerned that they could not be established by induction. Obviously it is absurd to talk of a statement such as this as a hypothesis (unless one had forgotten the way one had done something and was going through the possibilities in order to recall). What we are doing is to deny Wittgenstein's view that a causal statement is a hypothesis. We are doing this by separating the issue of establishing a causal law from making a causal statement.

So if a reason is a kind of cause what kind is it and does this have consequences for attribution theory?

A reason is a rational cause. Giving a reason explains an action by rationalizing it. As Wittgenstein put it, it shows a way to the action, it leads us to see some aspect of the action to be explained as valued by the agent. Davidson coins the term "pro-attitude" to cover wants, hopes, desires and so on. These are a subset of "propositional attitudes", the philosopher's term for beliefs, doubts as well as hopes desires and so on, that is, attitudes "directed to" propositions: Ibrahim believes that it will rain tomorrow. He holds that for a rationalization to be an explanation it has to invoke more than just the agent's pro-attitude, it

has to invoke (or at least presuppose) a means-end belief, a belief about how the desire should be satisfied. For Paul wanting to be kinder to the environment to be an explanation of why he switched his car to unleaded petrol we have to add, or presuppose it is true that, Paul believed that by switching his car to unleaded fuel he would be kinder to the environment. Davidson points out that it is generally unnecessary to cite both pro-attitude and means-and belief. Interestingly, Ellen Skinner, a developmental psychologist, has evolved a similar scheme but she has found it useful to use a triad adding a control-belief. So we also have: and Paul believed that he could switch his car to unleaded petrol.

To summarise this presentation of Davidson's theory; Actions are a class of events, events where intention is relevant. Reasons are a class of causes, they are rational causes. Reasons explain or rationalise actions, (mere) causes explain (mere) events.

1.2 Implications for attribution theory

I want now to apply Davidson's ideas to the ideas put forward by Buss (1978) and Kruglanski (1979). Buss opens his discussion with the following outline of his position:

> 1 Causes and reasons are logically distinct categories for explaining different aspects of behaviour.
> 2 Causes are that which bring about a change.
> 3 Reasons are that for which a change is brought about (eg goals, purposes etc.
> 4 Behaviour that happens to a person - that is nonintentional, that a person "suffers" - is an occurrence and is explained by both actors and observers with causes.
> 5 Behaviour that is done by a person - that is intended, that has a goal or purpose is an action and is explained by the actor with reasons. The observer may use causes and/or reasons in explaining action.
> 6 Thus, the kinds of attributions made depend upon what kind of behaviour is to be explained (occurrences or action) and the status of the explainer, (observer or actor).
> 7 Attribution theorists have tended to project an exclusively causal framework onto lay explanation and all behaviour and all explainers are thus confused and confusing regarding causes and reasons.
> 8 Progress in an area is, in part, dependent upon the adequacy of that area's key concepts. Attribution theorists need to become

> more self-conscious about the correct use of the terms cause and reason in the explanation of behaviour.

I think 1 is correct, behaviour seen as a mere event, Sheila sneezing, would be explained by a cause, behaviour seen as an action, Sheila fumbling for her tissues, would be explained with a reason. 2 is correct, but 3 unsatisfactory. I suppose we could say Bill switched his car to unleaded petrol for, for the sake of, his desire to be kind to the environment. Accepting Davidson's view that the reason includes a means-end belief means that Buss would have to talk of Bill switching his car...for what? for his belief that unleaded petrol is kinder to the environment? This seems awkward. Hereafter things go haywire. If by "non-intentional" Buss means that intention is not relevant then this agrees with our thesis. If he means "unintentional" then 4 would hold that Hamlet's stabbing Polonius behind the arras is not an action, I do not see how this can be.

5 Explaining an intentional behaviour with reasons is constitutive of that behavioural event being an action. Here we agree. Could an action, an intentional behaviour, be explained by causes which are not reasons? "Hamlet drank from the poisoned cup because..." can there be a cause which is not a reason but which enables us to maintain that Hamlet intentionally drank? I do not think so. If an event is explained without intention coming into the picture then that event is not seen as an action. Take Hamlet drinking from the poisoned cup. If we say Hamlet drank from the cup because he slumped under the table and the vile contents trickled into his open mouth then that event is not an action. If we say he placed himself in such a position that the vile contents... then it is an action.

An ambiguity that suggests itself earlier comes out: Buss seems to think that some behaviour simply is an action, some simply is an occurrence. The whole point is that whether it is an action or not depends on how it is viewed in the explanation. Some behaviours do seem to fall into the subclass of actions, taking a square root, and some do seem to be mere events, reflexes. I would say that if an observer (or an actor) explains an event, some behaviour say, without invoking intention then that behaviour is not an action, (is not seen as,

or represented as, an action). The view which Buss expresses here, that it is a conceptual matter that actors and observers must offer different kinds of explanation, is the basis for his view that attribution theory needs the distinction that he tries to draw. I have argued that whether some behaviour (or any event for that matter) is explained using causes or reasons depends on whether it is explained as an action or a (mere) event. It seems to me to be seriously mistaken to imagine that the logic of our languages forces observers of a behaviour to see it one way and the person whose behaviour is up for explanation in another. Buss is right when he says that these different kinds of explanation are logically distinct. Explain an event with reasons, bring in intentions, and the event is an action and the person behaving is an agent. I think he is right in an interesting and important way, when he suggests that the way in which questions are asked, and the status of explainers, matter. This is to do with the implications of being, or not being, an agent. These issues are moral, rhetorical, in short loaded ones, and are to do with whether on sees oneself, or another, as blameworthy or praiseworthy, or guilty or not guilty.

Buss can be taken to task for not making it clear that this is a conceptual finding, based on the logical properties of the concepts involved. Harvey and Tucker (1979) understand Buss to be putting forward a hypothesis. As I hope I have shown, his claim that we can conclude what kind of explanation a person will draw is a bogus one.

Kruglanski (following Buss in seeing teleological explanation as identical to reason explanation) agrees that since these explanations crop up in lay explanation then attribution theory should study them. He says two remarkable things.

1, That reason and causal explanations are part of an indefinitely large class of explanation types and that attempting to classify these would be a fruitless task. He considers the cause reason distinction to be on all fours with any other classifications of explanations that could be proposed for example, mechanical - nonmechanical; biological - nonbiological. This is remarkable because Kruglanski has not understood the way in which causes and reasons are distinct;

I would have thought that the attribution of agency to another is of key importance in our analysis of explanation.

2, That conceptual analysis of such notions as causes and reasons is tantamount to a "preoccupation with lay concepts". Perhaps Kruglanski feels that no events can be seen as actions in a science of psychology; a move that B.F. Skinner advocated. Isn't the "lay" idea of cause still present? Wasn't it through a preoccupation with lay concepts that such a move was made? I agree with Stratton et al. (1988) that we have to ask what purpose a conceptual analysis will serve. My purpose here has been to decide what expressions should be seen as causal ones: For an attempt to select "attributions" from an open field it is important to be clear about what we are looking for. In trying to describe explaining we should be as clear as we can be about the issues involved. I think that grasping the logic of explanation, the notion of agency with its shades of culpability and the rest, is important.

2.0 Sentential connectives in causal expressions

Given then, that some expressions are not be excluded from the analysis of causal beliefs on the grounds that they are reasons, I want to turn to the subject of how speakers express causal beliefs.

Some writers have suggested that a causal statement can be identified by the presence of a keyword, for example "because" or "therefore", or a phrase, such as "due to", (Castellon). Others have reported research based on a keyword identification (Antaki and Naji, 1987). At first glance this approach seems fine.

HA7 (01.26) I can't go anyway <u>because</u> it's not fit to live in yet.
HA7 (04.26) I hadn't expected it you know <u>so</u> that upset me.

Clearly, however, some expressions of causal beliefs do not contain a causal connective.

HA7 (04.26) The first week was fine we sort of got on with it.

Here it could be suggested that a connective is elided. The sense does not seem to be changed with the following substitution:

> HA7 (04.26)' The first week was fine <u>because</u> we sort of got on with it.

Are these cases of connective-free causal statements the exception or the rule? When connectives are present which ones tend to occur? How big a list would one need to capture most causal statements? How many false positives would result? Clearly words which can be used as causal connectives are not only used in that way, for example:

> TC8 (29.50) I don't mean to put it <u>so</u> extravagant as that.

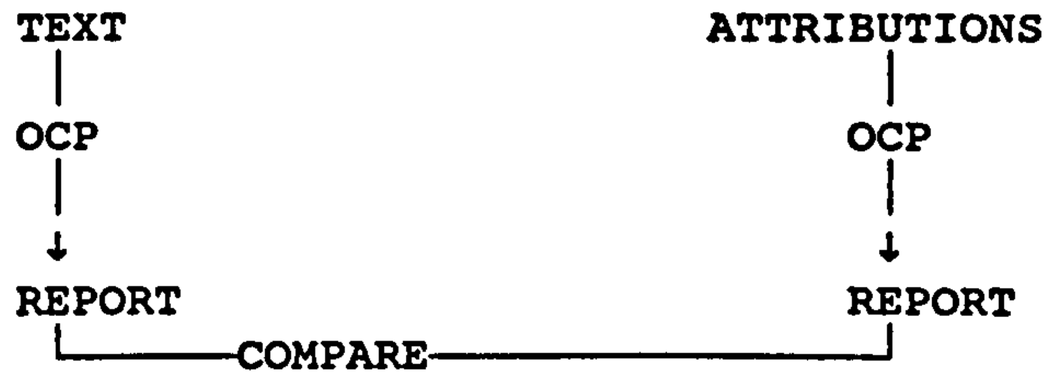
Answering these questions for a sample of verbatim material is, in principle, straightforward. What we require is a sample of expressions which someone judges to be causal ones. We could then look at the variety of causal connectives in the selected expressions. The answer will of course, be relative to that person's judgement, but it seems to me to be a sufficiently interesting question to ask what expressions are routinely recognised as causal ones. And the answer should enable an assessment to be made of the adequacy of using a machine to sample causal expressions in analysing a text.

The data used in the studies which I reported in chapter two and the LFTRC archives offer data of just this kind. Furthermore, since these are on magnetic disk a computer search could be used. A concordance program such as the Oxford Concordance Program (OCP) developed for linguistic and stylistic analysis is used in this study.[1] The program will search through a text and report all the occurrences of a specified list of keywords together with a certain amount of context. So a comparison can be made between the keywords/phrases present in the list of attributions with those present in the source text as a whole. This procedure is sketched in figure 3.1:

---

[1] I'd like to thank Lee Davidson of the department of linguistics and phonetics for his help in using OCP.

FIGURE 3.1 PROCEDURE FOR COMPARING CAUSAL CONNECTIVES IN THE SOURCE TEXT WITH THOSE IN THE ATTRIBUTIONS



This approach differs from the study reported by Altenberg (1984) in that here I can look at expressions noted by the coder without them having to contain explicit connectives at all. The use of a keyword list here is 1, just one way of isolating those sentential connectives which do contain explicit connectives; 2, looking at connectives over all noting which connectives were not selected.

The task of sending the files from disks formatted on a BBC Micro to the mainframe computer where a version of OCP is available is fairly straightforward using a file transfer protocol such as kermit. In the case of the this material, however, certain steps must be taken to prepare the text for analysis by OCP. Both the text and the list of attributions need to be formatted in various ways and in additional I replaced the participant's names with pseudonyms. Unfortunately there are numerous snags with the data available.

The list of extracted statements made by a coder provides an identification of that coder's judgement of what a causal belief is. When a coder makes a list of the causal statements they often paraphrase what the speaker says. Paraphrasing the connective itself, or the introduction of a connective where there was none originally, wrecks the chance of using the list as a sample of naturally occurring causal expressions.

For example the following attribution is reported

```
DS1E (24.58) You like pocket money because of the things you
can do with it.
```

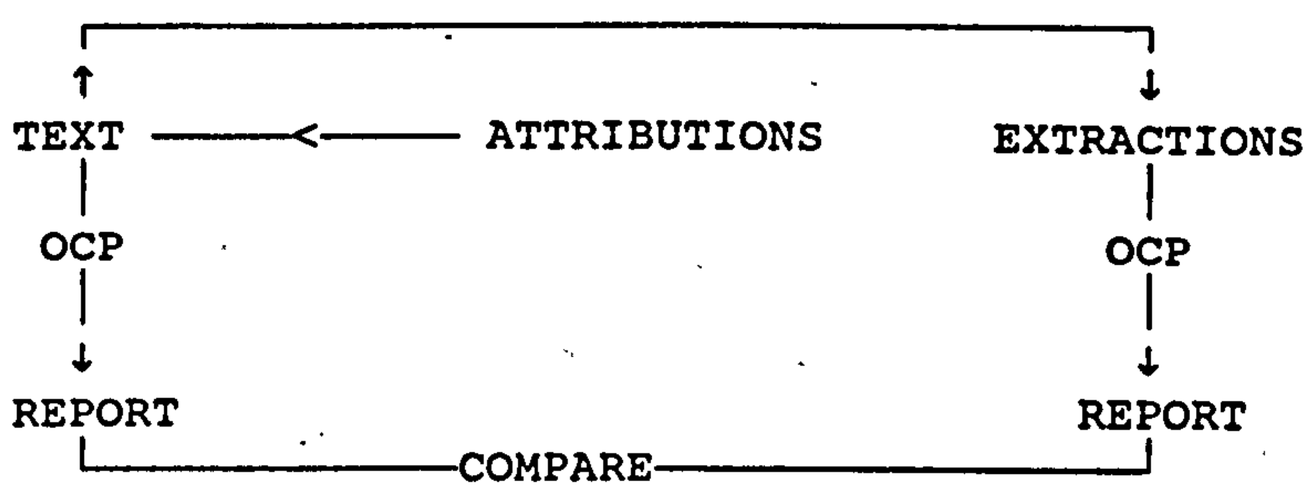
But the text reads:

```
DIGS1 24.58 Yeah.  Well, pocket money helps surely.  So you
like the things that you can do with pocket money.Mmm?
```

Fortunately the practice followed by most coders involves attaching the time of the statement. This helps us to locate the original utterance.

Method 1: Can the location be done by machine? The time recorded in the attribution can serve as a "tag" by which the original statement itself can be retrieved. By editing out all the times in the extracted list a file containing editing instructions, a macro, can be made which will modify those times in the original file. A further macro can then be run on the tagged file, picking out the tagged utterances and a certain amount of context.

FIGURE 3.2 PROCEDURE FOR COMPARING CAUSAL CONNECTIVES IN THE SOURCE TEXT WITH THOSE IN EXTRACTED PORTIONS CORRESPONDING TO THE ATTRIBUTIONS



Unfortunately the time itself is misreported in a small number of cases. In these cases, of course, the selection procedure will fail, almost invariably failing to find a corresponding time in the original list, a false negative. But these failures can be readily detected since the messages returned from the macro will indicate "No line(s) changed". I developed a test procedure based on this principle.

There are further problems. Sometimes the tag is inadequate. This is a problem in in the case of long uninterrupted stretches of speech. Here a whole crop of extracted attributions will share the same tag and only the one near enough the start will be extracted. This problem together, with the problem of a misreported time which happens to be a time of another statement, can only be detected manually. Both these errors will have the effect of producing a

false negative: the procedure will fail to find an attribution that was in fact detected.

Wrong speaker identified: The OCP identification of speakers is unreliable due to a disparity between the method used to identify speakers in the LFTRC data base and the method demanded by OCP.

Compound attributions: There is not always a one-to-one correspondence between attribution and utterance; sometimes more than one attribution is detected in a single utterance. Such cases affect attempts to find the proportion of any particular class of attributions in relation to the total. Here the total number of attributions should be adjusted so that each attributional statement is only counted once.

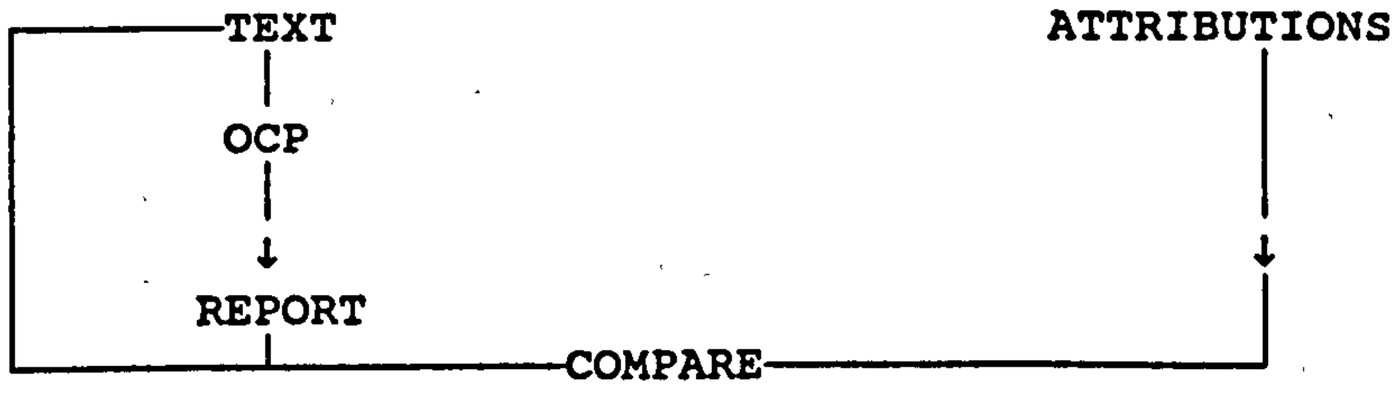
Unfortunately the files yielded by this procedure are simply inadequate. The fundamental problem is that the expressions selected often occur a long way beneath the time cited. For example, see the three attributions below and the corresponding test which the procedure extracted. Here the second utterance is retrieved successfully but the first one is not. The third attribution yields a redundant sample of text as well as the one desired.

```
 <T AR1>
((02:08)) <S MUM>  They're away , so I'm afraid that was out of
the question.  [coming to Clinic].

((03:47)) <S DAD> [I answered for him because]  he probably
doesn't know.

((04:40)) <S DAD>  I'm now managing director of the firm in
Leeds, otherwise I'd up sticks and back to Kent.
-------------------------------------------------------------
                        Yes
                .....and umm I think we were quite expecting
that they'd come as well.....
     ((102:05))  <S MUM>  + They don't live...  umm.....
((1$02:08)) <S DAD>  ! One's in London.....
     ((102:10))  <S MUM>  + One's in London.
 .....other one ----Polytechnic so...
          .....I see so there away
-------------------------------------------------------------
      Infact we'd better start further than that hadn't we?
 Where were you born Martin?
```

```
((103:45)) <S MUM>  +    Nottingham.
((1$03:47)) <S DAD>       I was just going to ask Martin.....
 ((1$03:47)) <S MUM>  + He probably doesn't know.
              .........no a good one.  We have one...  one boy
born in Colchester, and two born in Nottingham.  I've moved
-------------------------------------------------------------
Where were you born Martin?
((103:45)) <S MUM>  +    Nottingham.
((1$03:47)) <S DAD>       I was just going to ask Martin.....
 ((1$03:47)) <S MUM>  + He probably doesn't know.
              .........no a good one.  We have one...  one boy
born in Colchester, and two born in Nottingham.  I've moved
 round with my job, and we thought we'd got back home when we
were going back B-------- in Kent didn't we?  And
-------------------------------------------------------------  +
 Six and a half years ago.
((104:38)) <S DAD>        Yes.
((104:39)) <S MUM>        Six years last August.
((1$04:40)) <S DAD>     I'm now managing director of the firm
in Leeds, otherwise I'd up  sticks and back to Kent.  Umm..
and I think this has been unsettling don't you?          +
((1$04:48)) <S MUM>
 -------------------------------------------------------------
```

Method 2: An alternative is to compare the OCP report file from the transcript with the list of attributions manually, with reference to the transcript for help where necessary. This procedure is outlined in figure 3.3.

FIGURE 3.3 PROCEDURE FOR COMPARING CAUSAL CONNECTIVES IN THE SOURCE TEXT WITH ATTRIBUTIONS



2.1.1 Data

There are four groups of data in the LFTRC archives for which there are corresponding analyses of causal expressions as a result of previous work. The first group listed below is from my own analyses of the data reported in the first study of chapter two. The other three groups are from analyses carried out by other members of the LFTRC to whom I am very grateful for letting me scrutinise their work. Group (2), families carrying out tasks, is drawn from

data forming part of a study on the relationship between family dynamics and obesity in children. The data consist of transcripts from eight families, four with obese children and four without, carrying tasks in accordance with tape recorded instructions. Group (4) consists of interviews with families in their own homes carried out as part of a recent doctoral project at LFTRC.

TABLE 3.1 PROVENANCE OF DATA

	Provenance	Number of words
1	Families in therapy (1).	41 565
2	Families tackling tasks.	75 804
3	Families in therapy (2).	65 478
4	Families not in therapy, (Darlington Interview).	32 445
	total	215 292

Chapter two gives more information about the nature of the transcripts. Tables 3.1 and 3.2 below show the amount of time in conversation that they constitute and the number of attributions present.

2.1.2 Analysis

The method used is to specify a set of keywords/phrases and to see how many are represented in the selected attributions. What list should be used? Of interest here is the pair "because" and "cos" (Antaki & Naji, 1987) or the complete list produced by the linguists Quirk, Greenbaum, Leech and Svartik (1985). Appendix 2 contains an OCP command file of the latter list, this was used in the present study. The procedure is to compare the OCP output of all occurrences of because/cos with the list of all attributions and to score a "hit" for those occurrences which were selected by the analyser and a "miss" for those which were not.

2.1.3 Results

Table 3.1 and 3.2 summarise the data for each group of families.

TABLE 3.1 SUMMARY OF RESULTS FOR EACH GROUP OF TRANSCRIPTS AND OVERALL SHOWING
--NUMBER OF WORDS, NUMBER OF WORDS PER MINUTE (RWORDS)
--NUMBER OF ATTRIBUTIONS, NUMBER PER MINUTE (RATTS), NUMBER PER WORD (PATTS).

T	TIME	WORDS	ATT	RWORDS [per minute]	RATTS	PATTS
1	373.82	41565	461	111.43	1.23	1.109
2	466.67	75804	894	162.44	1.92	1.179
3	484.20	65478	766	135.52	1.58	1.170
4	239.93	32445	151	135.23	0.63	.465
	1564.62	215292	2272	137.88	1.45	1.060

TABLE 3.2 SUMMARY OF RESULTS FOR EACH GROUP OF TRANSCRIPTS AND OVERALL SHOWING:
--NUMBER OF "BECAUSE" AND "COS" UTTERANCES SELECTED BY ANALYST (BCH), IN TOTAL (BCT) AND THE PROPORTION SELECTED (PBC=100*BCH/BCT);
--NUMBER OF ATTRIBUTIONS (ATT) AND THE % CONTRIBUTION OF "BECAUSE" AND "COS" EXPRESSIONS (CBC=100*BCH/ATT);
--MEAN RATE OF OCCURRENCE OF "BECAUSE" AND "COS" EXPRESSIONS (RBC=BCH/TIME) AND
--NUMBER OF "BECAUSE" AND "COS" UTTERANCES PER MINUTE (RBCT=BCT/TIME*1000)

T	TIME [min]	BCH	BCT	ATT	PBC [%]	CBC [%]	RBC [per min]	RBCT
1	373.82	137	230	461	59.57	29.72	.37	5.53
2	466.67	257	316	894	81.33	28.74	.55	4.17
3	484.20	216	322	766	67.08	28.20	.45	4.9
4	239.93	54	85	151	63.53	35.76	.23	2.6
	1564.62	664	953	2272	69.67	29.23	.42	4.43

The overall number of because and cos statements in this data differs from the figure reported by Antaki and Naji (1987). For a 200,000 word sample from the Lund corpus they reported 666 occurrences of because and cos statement, a frequency of 1.06 per thousand words. This is presumably due to different choice of words for the speakers in the two samples. Antaki and Naji report that the sample that they used consisted of 140 speakers, of whom 67 were "academics" "and the rest were in middle class occupations, such as 'banker',

'doctor', 'teacher' and 'nurse'for the number of "because"/ "cos". All speakers were over 20. My sample included children and people in other kinds of work such as a mechanic and a clerical worker as well as some people who were unemployed.

From the point of view of looking at the prevalence of attributions containing because or cos column six of table 3.2 is of interest. This shows that "because"/"cos" only constitutes 29.23% of the attributions selected.

Of course not all the occurrences of this connective were picked out by the analyst. A worst case would be if we assume that all occurrences of "because/cos" are "attributions". This would then make the total number of attributions ATT + (BCT-BCH). In other words, (BCT-BCH) were attributions, but were missed by the analyst. So then 289 attributions were missed out of a total of 2561. In this situation, the proportion of attributions to which "because" or "cos" contribute would be 953/2561 = 0.372, 37.2% This suggests that Antaki and Naji (1987) will have missed a high proportion of attributions in their study. If the speakers studied by Antaki and Naji are using a wider range of constructions to express causes then this strengthens the case that their method of selecting causal expression misses many that are present in the data. Of course I cannot say whether or not this would bias the findings which they report on the basis of their method of collection.

Why were some expressions missed and what features are by the rest of the expressions selected? Looking at the occurrences of because/cos which were not selected we find that many reveal good reasons for being omitted by the analyser: 1, They may have been uttered by the therapist or interviewer whose speech was not included in the analysis which we are studying. 2, The explanation was incomplete, for example the speaker my fail to complete a causal expression:

```
SR1A
<A 12:51> <S DEB> I don't like football either.............
     <A 12:55> <S PETER> + I don't like...I don't like ballet
 because it's.....................don't like school.
```

```
                                    ..................but I like majorettes.
And I like ballet.  That's what I..........
```

or there could be a problem with the text:

```
PT1A <15:35> <S ROGER> +  Yes.  [inaudible] 'Cos he's getting
married to Sheila.
```

A further interesting problem develops. In the case of families solving tasks. Here we find causal utterances of relating to the task itself.

```
CI1A <29:36> <S DAD>   Yeah, but are these three at the bottom
there supposed to be yellow because they've got no
red....no...no reds.
```

These have informally been referred to as "rules". In practice some appear to be selected, others not, for example:

```
KT1A <29:05> <S DAD> !      Why not?
<A 29:07> <S MUM>  +     Because the mauve's there and the
yellow's there.
```

2.1.4 Other connectives

What of the other connectives? "As", "so" and "for" occur in great numbers, many of which are clearly not causal. For example:

```
AR1T  <A 11:10><S MARTIN>  They are so close together and so
totally different that they don't get on terribly well.  But I
mean that's between the two of them, they get on with that, it
doesn't usually involve Martin does it?

AR15 <A 08:48><S DAD>                ...  And he says he wants to
be regarded as a normal eighteen year old but he can't convert
two and a half yards into feet and inches...

HA1T <A 30:32><S  MUM>  ...I'd get up and  get Mary  and
Steven dressed and sorted out and then start  on the sweet
trolley  as  soon  as Mary went to bed for the  morning
nap.......
```

Analysing these in the same way as because/cos above is not feasible because of the large number involved: there are, as it were, too many false positives. This raises the issue of selecting the verbatim segments of speech used by the speaker. As I have shown above there is no economical fast way of achieving this. Furthermore it seems that this cannot be done on any simple systematic basis as we continually find attributions drawing on large areas of text.

For example, in the attributions:

<06:12> <DAD> If somebody says do you want to go out for a drink tonight with it being tuesday, mid week, Ithink no, I want to do something at home.

<06:39> <DAD> Unless I was really enthusiastic about it or I'd had a bad day and wanted to shut off I'd probably say no, Idon't want a drink or go to the pictures.

Correspond to the following piece of text:

CL1T <A 06:08> <S THERP> So would you say that you weren't keen or...
+
<A 06:12> <S DAD> Oh no, no, no, it's er.. I think that it's er..... a case of... I don't know... how would you describe..... it even boils down to if er..... if somebody says oh do you want to go out for a drink tonight, with it being Tuesday, not that there's 'owt on telly that I want to watch but... sort of mid week an I think... na.. I want to do something at home... and I'd even put off going out for a drink, wouldn't I? truthfully.....
<A 06:39> <S MUM> ! Yes
..... through week you know, unless I was really enthusiastic about it or I'd had a bad day and I wanted to. to totally shut off. Umm..... I'd...I'd probably say na I don't want a drink you know.... or go pictures I don't go to pictures or 'owt like...
dy.... umm....

It appears that we cannot opt for anything in between the entire text and the paraphrased attribution in any other than an arbitrary way. Earlier in the same text there is a string of examples showing a range of statements selected as attributions, but which lack the causal connectives discussed by Antaki and Naji. The attributions recorded follow the piece of text in square brackets

<T CRL11T>
<A 03:40> <S MUM> + I've basically been in clerical but... I've done waitressing and all sorts of other jobs.... which I prefer actually. I think this... the job that really gets me down just lately..... you know... I think that's what it is, definately.
+
<A 03:59> <S DAD> + We're feeling these stress points and..

<A 04:02> <S MUM> Plus working together, you know, living together and working together the same office incidentally... umm. I don't think... I think where together too much.... constantly. It doesn't seem to bother Barry. Well it's not apparent with him but.... I crack a bit quicker than you do.

[I think it's the job that really gets me down just lately]
[Plus living and working together in the same office I think we're together too much]

<A 05:11> <S MUM> Anything that upsets his little daily routine bothers him.

[Anything that upsets his little daily routine bothers him]

<A 06:54> <S MUM> No let's face it in town I'll stop with any market researcher and umm.. and Susie and I stop with them all don't we... testing crisps and everything. I like to take part in something if it's helping somebody.

[I like to take part in something if it's helping somebody]

if he came up.... and said right are ya going out then I'd go.

[If he came up and said right are you going out then I'd go]

So in addition to constructions involving "if" there is a class of expressions which contain verbs with a causal semantic component such as "gets" and "bothers" in these contexts.

Note that "why" can occur as a question or as a causal connection as the "that's why" in

<A 13:35><S MUM>
> Like that...Mary no..[talking to baby]
.....to the extent that's why the pressure is on

2.2 Attributions and questions

In providing a rational for their choice of the Lund Corpus Antaki and Naji refer to Stratton et al (1986) and remark that in this data causal statements were specially sought. This is not in fact true. Perhaps Antaki and Naji consider therapy to be fundamentally about therapists searching for causal beliefs. The issue of whether or not attributions occur as responses to questions from others can be addressed in an approximate way with this data. The number of times that "why" occurs, uttered by anyone, let alone the therapist is small compared to the number of attributions overall. Looking at other forms of interrogatives "what made you..." we again find few of these. Overall "why"

occurred, not necessarily as part of a question, 237 times. Since a total of 2272 attributions were recorded the ratio of "why" to attributions is 0.104. It does not appear then that in this data the explanations offered by speakers were specifically elicited by the therapist or interviewer.

2.3 Discussion of results

It strikes me as important to make clear three limitations which should be bourne in mind when assessing the results of this study. Firstly there are two problems with the data itself: there may be undetected errors in hearing what the speakers said or in its spelling. Such errors won't affect my results subtantially however. More importantly, the study looks at just those statements which struck a certain group of people as being causal ones. In the absence of any alternative I don't see this reliance on the analyst's intuition as a principled problem. The problem is that other analysts may have different intuitions. In this sense the subjects in my study are the analysts and I am looking at what expressions strike them as causal ones, an interesting enough project I think. The findings are relevant to attempts to pick out causal statements by machine. I have shown, indeed quantified, some of the problems with such an approach.

3.0 CONCLUSION

In this chapter I have tried to address two issues which are important in attribution theory and in the study of explanations in general. Conceptually, I have tried to show that reasons are a species of cause and to address this to a debate on reasons and causes in attribution theory. Empirically I have looked at some of the features of utterances which led to them being classified as causal ones.

In that study I have tried to develop and clarify points raised in the LACS Manual and in the study of Antaki and Naji (1987). I have shown that a machine extraction process would not find many of the "attributions" detected by a human analyser, but that it could be of help. The keyword approach used by Antaki and Naji would only sample at most half the attributions present. Though as they remark there is no theoretical reason, and no evidence here that

their finding (that single actions of another person rarely occur as topics of explanation) should be different when other causal connectives are used. This is a subject which could be taken up empirically. A topic for further study is explored by Altenberg (1984). Altenberg looks to considerations of discourse structure to explain the range of expressions which he finds. That is, that discourse structure may constrain speakers choice of causal expression. A more radical issue is the issue of whether or not causal beliefs themselves are discursively formulated. In the next chapter I want to consider how conversational structure has been studied and whether or not such approaches are helpful to an understanding the context-specific pressures on a speaker.

CHAPTER 4: METHODS OF ANALYSING SPOKEN DISCOURSE

In this chapter I pursue the idea that in analysing causal beliefs in naturally occurring discourse it is utterances, rather than beliefs that we are studying. Now utterances are situated, that is, they occur in specific contexts. By context I am specifically concerned with the communicative, rather than, say the physical context. In order to examine the sense in which what a speaker says is conditioned by the context in which they speak I need to take up a prior question: what account can be given of the communicative context? in other words, how can utterances, speech, be analysed? I review seven approaches to the analysis of conversation which have developed in various disciplines. For practical and theoretical reasons I argue that the approach generally called "conversation analysis" is the best equipped to guide empirical work in the field, at least for my purposes, and I discuss some of the findings and methods which have emerged from this subdiscipline. Practically, this approach does not rely on the classification of types of utterances as a means of analysis: it tries to use material present in detailed transcripts of conversation. Theoretically, it does not impose a model of discourse structure nor speaker's methods onto the data. This approach stimulates the studies presented below in chapters five and six. I discuss it further chapter eight.

I propose and discuss the term "communicative constraint" as the most general term to describe how what a person can say is conditioned by the context or situation in which they are.

1.0 Introduction

1.1 Utterances as situated events

In what sense are a speaker's utterances constrained by the situation, or context, in which they occur? So far we have looked at causal statements in naturally occurring talk and we have considered the impact of this domain for attribution theory in particular. In this chapter I want to continue the theme of a transition from causal beliefs to causal utterances by examining how the fact that utterances are situated events can be studied. Utterances are situated in the

sense that they occur in specific times and places. This feature is dramatic in the case of deixis, or indexical expressions (Bar-Hillel, 1954). An indexical is an expression which contains an index or pointing term and which relies on the fact that the utterances occurs at a certain time and place for its sense. Stock examples of such expression are pronouns and temporal expressions.

"I am called John" is true when uttered by me, and others with that name. The truth value depends on who says.

"Tomorrow is 12th August 1989" is only true when uttered on a certain day.

Although we generally analyse such expressions in terms of the situation being critical there is a counter part to this in that the speaker has to have an understanding of the situation in which they speak. In the case of the communicative environment this ability has to be acknowledged as a considerable accomplishment. A speaker knows how to embed utterances in contexts in order to bring about certain things. Habermas (1970a) calls this "communicative competence". In so far as speakers are responsive to features of the situation we must acknowledge that the situation places constraints on what speakers can say.

I propose to consider pressures which operate in speech situations as "communicative constraints". Perhaps this term sounds unduly pessimistic, but I don't intend "constraint" to imply solely coercion, such constraints are also enabling. A standard account of the interpretation of a problematic expression is that the interpreter has to visualise a context within which the expression makes sense. In the case of an ambiguous expression we often say that the context constrains, or narrows, the possible meanings that the expression can have, thus Ricoeur writes:

> What is important for the present discussion is that the polysemy of words calls forth as its counterpart the selective role of contexts for determining the current value which words assume in a determinate message, addressed by a definite speaker to a hearer placed in a particular situation. Sensitivity to context is the complement and ineluctable counterpart of polysemy. (1981,p. 44)

Furthermore, we also talk, in at least some cases, of an utterance of one speaker constraining the next utterance of another speaker. Indeed I think that the concept of a reply involves some notion of a constraint. To be a reply an utterance must be interpreted in such a way that it means something with respect to a previous utterance. For example if Jones asks Smith when she's going to take her holiday, possible replies are under the constraint of containing some temporal index or, failing that, a holiday relevant detail, or a reference to the situation of the questioning.

J: When are you going on holiday?
S: Not until June 15th.
S: I haven't decided yet.
S: It's none of your business

I think that a response such as:

S: I'm going to the cinema tonight

Would not usually be recognised as a reply. Of course as I said, it could be counted a reply if it could be interpreted in a way that meant something to the questioner. I will discuss these issues further when I examine the idea of norms. Although I have stressed that I do not want to restrict the sense of constraint to situations of dominance and although I shall not study it here, the coercive connotation is important and should not be forgotten.

In this chapter I propose to ask, 1 what formulations do we have for thinking about the relationship of utterances and their contexts and 2, what empirical research has been done on this? In chapter one I discussed some of the ways in which approaches to the social and human sciences and studies have oriented themselves to studying discourse. Some of that discussion will be relevant here, for example, ethnomethodological investigations have developed an approach to analysing conversational structure.

In what follows I want to 1, make a brief critical review of some approaches to studying spoken discourse which might be expected to be of use in my analysis of communicative constraints; 2, draw together some of the ways in which the idea of constraints has been used.

2.0 Approaches to analysing talk

In this section I propose to review seven approaches to studying talk. As with my review of approaches to the place of language in researching day-to-day understanding in chapter one, these approaches are not all on an equal footing, differing in their objectives. The first two are approaches that have emerged in social psychology; the second is an approach which has arisen in linguistics; the third and fourth are philosophical analyses which have stimulated some empirical work; the sixth has arisen in anthropology/sociolinguistics and the seventh in sociology.

2.1 Social Psychological Studies: Experimental

The distinctive feature of a study which proceeds by an experimental method is that conditions are manipulated so that the effect of different conditions (the independent variable) on something else can be observed. Many researchers have felt that the experimental method is the hallmark of science. Two things, however, bear on an experiment. The state of knowledge about the phenomena observed needs to be quite advanced so that a manipulation can occur at all. Secondly we need to be in a position to say what variable we want to test. Several researchers in the field of conversation have argued that naturally occurring data are important, I agree with them. On this issue Goodwin (1981) cites Condon and Ogston (1967, p. 221)

> the need to control the variables in experimental method tends to modify the process under investigation. In human behaviour, it is quite often not even clear what the variables are, such that they could be controlled. What is required to some extent is a method which could investigate and make relatively rigorous, predictable statements about a process without disrupting the process too severely.

Goodwin also cites Kendon (1974, p. 150)

> In all these cases, however, the investigator has studied only those features of the listener's behaviour he has determined in advance. The listener is always giving a controlled performance, where what he does and when he does it has been decided upon beforehand as part of the experimental design. We know remarkably little in

> a systematic way, about what it is that speakers ordinarily do, and how what they do is related to what speakers do.

2.2 Social Psychological Studies: Observational

In non-experimental studies social psychologists have tended to follow a procedure capable of yielding quantitative data. The method widely adopted is to choose a set of categories and to then note how often they occur in a scene. This approach is the basis of content analysis and of the procedure adopted by Bales (1950). There are two limitations of such a procedure: firstly, with respect to categorization, what categories are to be used? Secondly a simple frequency count of these categories fails to grasp the structure of the interaction or conversation.

Observational studies of conversation in social psychology have tended overwhelmingly to be carried out along the lines of content analysis. This proceeds by scoring the occurrence of selected phenomena in a stretch of data. The results are frequency counts. In a sense the studies reported in chapter three are content-analytic in so far as I am interested in the occurrence of certain categories, namely different causal connectives in transcripts and in analyst's selections of causal attributions. There are two limitations of this approach, which I shall argue, render it unsuitable for my purposes. Firstly there is the problem of the categories themselves. In my study of causal connectives the point of the analysis was explicitly to look at the occurrence of these terms. It is typically the case in a using content analysis that the researcher needs to choose phenomena as an index of something else. In a study which I will report below (Duncan and Fiske, 1977), the investigators didn't even know what phenomena would be of interest to them. Furthermore, in my study the categories can be defined precisely and distinguished mechanically. Firstly, there is no doubt about whether or not a "because" or an "in order to" is occurring. Secondly, there is no problem about distinguishing these from each other. The second major limitation is that in compiling a frequency count the structure of the data is unanalysed.

Duncan and Fiske (1977) report what they call an "external variable" study. This name seems to have been chosen for two reasons. Firstly the variables studied are defined etically, that is they are objectivistic. Secondly the variables are to be related to identify relationships between them and other measures, such as length of turn and speakers' judgements of whether or not the conversation was sociable. They hypothesised that personality would relate to behaviour in conversation. They find however, (p. 123):

> Our substantive findings from our studies of correlates of acts in interaction do not impress us. While the number of observed relationships was large, it formed a small proportion of the total number of correlations computed. In other words, the proportion of large correlations was not greatly above the proportion which would be expected by chance.

I have trouble understanding the point of this kind of analysis. It seems to be the case that research is motivated by finding data on which to exercise methodological ideas rather than to explore interaction.

2.3 Exchange Structure

The approach to the analysis of talk which is most closely rooted in linguistic theory amongst the analyses which I will consider in this brief review originated in the English Language Research Group at Birmingham University (U.K.). Important presentations of this groups work are Sinclair and Coulthard (1975) and Coulthard and Montgomery (1981). The intellectual orientation of the group can be summarized as follows: spoken discourse consists of a number of units which are hierarchically organised, units can only fit together in certain ways. Taylor and Cameron label this a "slot and filler" approach and emphasise its similarity to Pike's theory.

I have already indicated why I think that the isolation of units is problematic. There is nothing in this approach to justify how the stream of speech can be segmented into conversational units. The motivation behind the organization account sought by workers in this tradition is that there are quasi-grammatical rules which will generate stretches of well-formed discourse.

Writing on the "motivating analysis of exchange structure" Stubbs (1981, p. 107) remarks that

> Linguistics has traditionally been concerned with characterising well-formed versus deviant strings, that is with stating the constraints on the distribution of units such as phonemes and morphemes. The basic aim is to predict the correct surface distribution of forms, and the basic assumption is that any given string is recognisably well- or ill- formed, with only a few doubtful cases if any.

He continues, saying that grammaticality too, fading hopes of transformational grammar notwithstanding, is a meaningful concept. What of discourse?

> On the face of it, the concept of well-formedness also applies to discourse. (p.108).

Taylor and Cameron (p. 74) point out that the proposed rules, for example, that an "Exchange" is made up of a sequence of "Initiation" + "Response" + "Feedback" are just descriptions of the patterning of discourse, not rules. Furthermore, Stubbs needs to invoke a competence-performance distinction in order to explain departures from the norm. This is problematic since it leaves unexplored the data that we actually find. Taylor and Cameron go on to point out that it is not clear whether or not these principles are rules in the grammatical sense, or whether they are followable prescriptions which speakers choose to follow rationally.

This approach may offer interesting descriptions of speech phenomena at a fairly coarse level, descriptions which could be of value for specific ends in many teaching environments. It may be useful for people learning English as a foreign language to know that typical service encounters in the U.K. the person behind the counter may open with a question like "who's next" followed by a request to know what the customer requires.

Of course the reason why this knowledge would be useful is so that the language learner has the social know-how to get along smoothly. What an account of regularities falls short of offering is a grasp of what these regularities consist in: I don't detect either an analysis of how contexts may constrain speakers. This reason, along with my doubts about how move tokens are to be identified, renders this approach unsuitable for my purposes.

2.4 Speech Act Theory

Speech act theory originates in a series of lectures, the William James Lectures, given by the Oxford professor J.L. Austin at Harvard University in 1955. As it happens the next of the philosophical approaches which I want to consider, H.P. Grice's analysis of conversational implicature, also originates in a series of William James Lectures. The lectures were published after Austin's death in 1962 as a short book entitled How to do things with words. Although another philosopher, John Searle has amplified and systematized Austin's work. I want to start with a consideration of Austin's book.

The title is apt for Austin's objective is to show the sense in which utterances are used to do things, to bring things about, rather that to describe how things are. He starts his discussion with a consideration of utterances which would be classified as statements on grammatical grounds but which, unlike usual statements 1, do not report or describe anything and are not true or false; 2, when utterred are part of doing something, which would not normally be considered to be reporting or describing.

Examples are:

> 'I name this ship the Queen Elizabeth' uttered in the course of naming a ship, or
> 'I bet you sixpence it will rain tomorrow' (p. 5).

Austin proposed to call such utterances "performative utterances", or just "performatives". These are contrasted with "constatives", his name for statements which report or describe and which are true or false. If such utterances are not true or false are there any constraints on them at all? For a naming of a ship to be a naming of a ship certain things need to be the case, certain conditions need to hold. Austin call such conditions "felicity conditions". After analysing these he goes on to show that his isolation of performatives was really a didactic step and he proceeds to argue against the distinction between constatives and performatives showing that in speaking three acts take place.

The theory that emerges proposes that there are three kinds of acts performed when an utterance is made, locutionary, illocutionary and perlocutionary acts. The locutionary act is the act of saying whatever one does, the illocutionary act is performed in saying something, for example ordering something and the perlocutionary act is brought about by speaking, for example offending someone. Attention then focuses on illocutionary acts and it is these that get referred to as "speech acts" in post Austin work.

Searle's contribution is to systematize Austin's in three ways, 1, suggesting a typology of speech acts, 2, offering a conceptual analysis of what such acts presuppose 3, exploring, formally, the logic of speech acts. It is primarily the first contribution that empirical researchers have drawn on. Let us assume for a moment that we have a well defined set of speech act types and that we can isolate, from transcript what speech act is occurring. We could then apply this to an analysis of causal explanations, for example, distinguishing causal utterances occurring as question, commands etc. Such a project might yield interesting results, but I don't think that it is a project which we could ever carry out because we are not in a position to make the categorizing step.

There are two reasons for my pessimism about this. Firstly, the reason that emerges when Austin and Searle raise the issue of a typology. Austin remarks that a standard dictionary of English will contain between one thousand and ten thousand speech act verbs (p. 149). He proposes a set of families instead, an approach echoed by Searle (1976) although Searle distinguishes illocutionary acts from illocutionary verbs. Whereas he holds that illocutionary acts of differents kinds, or speech acts of different force, are distiguished on fundamental conceptual grounds and are part of all languages, illocutionary verbs are contingently part of a language. So Searle's approach to classification is to consider the conditions constitutive of speech acts. In this way he reaches a set of five speech act types. These are assertives, directives, commissives, expressives and declarations.

Assertives: commits speaker to the truth of the proposition, examples are statements;

directives: speaker intends that the hearer do something; examples are instructions, commands;
commissives: commits the speaker to some future act,
examples are promises;
expressives: speaker makes attitude knownfor example thanking or deploring;
declarations: bring about a congruence between the proposition content of the speech act and some state of affairs, examples are the words utterred in marriage ceremonies or in granting a pardon.

My second reservation is one that is emphasised by Taylor and Cameron namely that this account is etic, do speakers understand utterances in this way? Is it fundamental to conversation that they do? Although Searle's conceptual analysis is appealing I don't think it helps us interpret what speakers mean. Taylor and Cameron produce a clear illustration of some problems in an attempting to base an analysis of discourse on a taxonomy based on speech acts. This is a simplified extract from Edmondson (1981, p. 176)

1	X:	well er I'm terribly sorry but er	[apologise]
2		I'm afraid you're in my seat	[complain]
3		you've moved my books	[complain]
4	Y:	oh dear	[exclaim]
5	X:	and papers	
		you must have realized that somebody was here	[complain]

Taylor and Cameron reproduce a somewhat longer stretch of the discourse that this is taken from and query Edmondson's application of his own criteria. The definition for "complain" starts:

> S wishes H to believe that S is not in favour of H's having performed an act A, as being against the interests of S. (p. 143)

They query the appropriateness of categorising utterance 5 in this class and suggest that another category "opine" might be more appropriate. The definition for "opine" starts:

> As with the Claim [another category] above, S wished that H believes that the information contained in the locution by means of which the Opine is made is true. An Opine differs from a Claim concerning the nature of the information transmitted. (p. 145)

(Edmondson has a problematic fact/opinion distinction in mind).

This account however is only based on speech act theory in so far as it assumes that there are discrete speech acts. The fundamental notion of there being constitutive rules for speech acts to be what they are. If the possibility of

classification is not helpful to us in our attempt to analyse stretches of talk does this idea of there being constitutive rules help us to analyse conversations? Some users of ideas inspired by speech act theory have believed that it can:

Edmondson (1981, p. 190) in Taylor and Cameron (p. 59)

> It should be possible to formulate a set of discourse formulation rules which would recursively enumerate an unbounded number of interactional structures.

Clarke (1983, p. 106) op cit,

> The rules which generate all and only the speech act sequences which are sensible conversations are ... like the rules which generate all and only the morpheme strings which are well-formed sentences.

I think that it is merely incidental that these writers have claimed a basis in speech act theory for their work. There is nothing in speech act theory to support such claims. In this sense it is a little disingenuous of Taylor and Cameron to include these views in their chapter on speech act theory. Recently Searle (forthcoming) has actually based an argument against the possibility of giving an account of conversation structure by arguing that no rules analogous to the constitutive rules of speech acts could be applied at the level of conversations. I shall include some of his discussion of Grice's maxims and of Ethnomethodological work in my outline of these areas below, see section 4.0, Communicative Constraints. I think we have to draw a distinction between putting forward a taxonomy of speech acts and suggesting definitions or categories aimed at selecting speech act types from open discourse.

A classificatory account would only be possible as part of a theory of conversational structure for identification of speech act type would need to draw on features outside the utterance to be identified. Since Searle resists the possibility of an account of conversational structure it follows, if I am correct, that he cannot be concerned with the very point that people seeking to apply speech act theory to naturally occurring conversation have had, namely identifying speech act types.

In summary then, despite its merits in clarifying the nature of certain paradigm utterances, in emphasising that in speaking things are done and in drawing some

useful distinctions, speech act theory is not helpful in the investigation of the context-specific pressures on a speaker. Firstly, there are principled and practical objections to attempts to categorise naturally occurring utterances into speech act types. Secondly, understanding conversation structure in terms of speech acts is forlorn because speech act theory does not have the conceptual resources to carry out this task. I think that it is clear that neither of these are goals of the project to which Austin and Searle contributed.

2.5 Conversational Implicature

Grice holds that speakers follow a set of principles or maxims when they speak, and that it is this which gives conversation its regularity. These maxims differ slightly from rules, I shall have more to say on this point.

The argument of Grice's William James lectures is available in two papers, "Logic and conversation" (1975) and a follow up to it (1978). Grice wants to show, against the apparent contrast between the formality of an argument laid bare in formal logic and the casual looseness of everyday talk, that there is a logic to conversation. He invents the following exchange:

> Suppose that A and B are talking about a mutual friend, C, who is now working in a bank. A asks B how C is getting on in his job, and B replies, Oh quite well, I think; he likes his colleagues, and he hasn't been to prison yet. At this point, A might well inquire what B was implying, what he was suggesting, or even what he meant by saying that C had not yet been to prison. The answer might be any one of such things as that C is the sort of person likely to yield to the temptation provided by his occupation, that C's colleagues are really very unpleasant and treacherous people, and so forth. It might be quite unnecessary for A to make such an inquiry of B, the answer to it being, in the context, clear in advance. I think it is clear that whatever B implied, suggested or meant, etc., in this example, is distinct from what B said, which was simply that C had not been to prison yet. I wish to introduce, as terms of art, the verb implicate. (1975, p. 45).

So implication is insinuation. Certainly Grice has identified a feature of conversation inference which is clearly very important: participants in conversation have a "know-how" which enables them to reason about what is going on. To give an account of this phenomenon would be to describe an important

human capacity. Grice focuses on what the speaker does, rather than the hearer's capacity. He holds that implicature is related to general features of discourse. It is in the process of trying to say what those features might be that he proposed the well-known maxims. He puts forward what he calls the cooperative principle (CP)

> Make your conversational contribution such as is required, at the stage at which it occurs, by the accepted purposes or direction of the talk exchange in which you are engaged. (1975, p. 45)

Allowing such a principle, he goes on to list four categories which contain more specific maxims. Following Kant he labels these categories quantity, quality, manner and relations.

Category of quantity
Maxims: 1, "Make your contribution as informative as is required (for the current purposes of the talk exchange)."
2, "Do not make your contribution more informative than is required."

Category of quality
Supermaxim: "Try to make your contribution one that is true"
Maxims: 1, "Do not say what you believe to be false."
2, "Do not say that for which you lack adequate evidence."

Category of relation
Maxims: 1, "Be relevant"

Category of manner
Supermaxim: "Be perspicuous"
Maxims: 1, "Avoid obscurity of expression"
2, "Avoid ambiguity"
3, "Be orderly"

Speaking in terms of conversational goals seems a plausible way of understanding the structure of conversation. Unlike Clarke and Edmondson who favour a rule based model here we acknowledge that participants in conversation have choices and that constraints in what they say relate to having an interactionally successful conversation. Of course Grice's principles, though they identify something important, are demonstrably not always followed. Indeed Grice himself has an interest in how they can be flouted. What motivates speakers when they do go along in a way that these maxims describe? The method projects a rationality onto the conversation. To be sure I think that there is rationality in conversation, but the level at which Grice's maxims operate tells

us little about member's methods - why not use conversations as a source of conversational inference? This is one way of interpreting the ethnomethodological approach to analysing talk: conversation analysis. Before considering this I want to return to another approach which I reviewed in chapter one.

2.6 The ethnography of speaking

Research projects carried out under this title have used a variety of methods to collect and analyse data. What is, I think, valuable and distinctive about this approach is its orientation, namely seeing communication as an aspect of the cultural knowledge that members of a community have. In chapter one I presented two pieces of work which have been carried out in this tradition. In Frake's study of Subanun drinking ceremonies I tried to indicate how Frake attempted to exhibit the cultural know-how that a participant needs in order to take part in such an event. Frake says that " the cultural pattern of drinking talk lays out an ordered scheme of role play through display of verbal art "(1964, p. 131) In the study of code switching in Norway carried out by Blom and Gumperz we saw how a phonologically detailed analysis of conversation showed how situational factors appeared to influence the speech code that a speaker adopted. This provides a vivid example of what might be called a communicative constraint. Gumperz does in fact use the term "constraint" here and in other cases. I discuss the uses to which this term has been put section 4.0 below.

2.7 Ethnomethodological Conversation Analysis (CA)

In chapter one I outlined the role of language in the approach to sociology started and named by Harold Garfinkel. A detailed concern with conversation as place where member's methods can be studied is essentially due to Harvey Sacks and his pupils, although Goffman is often also evident as an influence. Having already sketched the back-drop to this approach I would like then to indicate the style of analysis followed by this group. There are now several edited collections of papers on CA, Sudnow (1972), Schenkein (1978), Psathas (1979), Atkinson and Heritage (1984), Button, Drew and Heritage (1986) Button

and Lee (1987) and several book-length studies such as Atkinson and Drew (1979) on language in law courts, also Goodwin (1981) and Heath (1986) which extend analysis to body movement. In addition some useful expositions on CA have appeared recently. Notably Heritage (1984, 233-292) for a detailed discussion and (1988) for a succinct account. Heritage has also reviewed the CA literature, Heritage (1985, 1989). Levinson (1983, 284-370) includes an account of CA. Inevitably Taylor and Cameron offer a critical discussion (99-124). Hopper (1989) provides a straightforward account of the differences between CA and social psychological approaches to conversation and warns against the possibility of a simple synthesis of these two disciplines. Sharrock and Anderson (in Button and Lee) reach a similar conclusion in their more theoretical discussion.

Because of its concern with repeated readings of transcripts of naturally occurring conversation and for its disdain of theory construction CA is often perceived to be atheoretical. Whilst the reading of transcripts is atheoretical in an important sense the project overall: the choice of conversation as data, the conception of the participants in conversation, the role of the analyst, the avoidance of premature theorizing itself are all supported by a coherently followed, though not fully articulated theory, namely Garfinkel's ethnomethodology.

I want to try to pick up my discussion of ethnomethodology by looking at Sack's use of conversation as data for studying social organisation. Often Sacks adopted an approach somewhat close to what I outlined above as the ethnography of speaking. For example in "Hotrodder: a revolutionary category" (Sacks, 1979) he discusses member's knowledge of how a hotrodder differs from other car owners and how distinctions are articulated. We come closer to the style of work so distinctive of conversation analysis in his paper "On the analysability of children's stories" where the ordering of the two sentences of
"The baby cried. The mommy picked it up."
is part of Sack's analysis. Sacks is concerned with how this pair of sentences is heard as a little narrative in which the "it" refers to the "baby" and the "mommy" is taken to be the mother of he child.

Attention to the sequencing of utterances, or "turns" as they are called in this literature, has been called the "analytic dynamo" of CA (Button and Lee). Consider the analysis of a turn in a conversation. The orientation of workers in the field of CA is to point out that the next turn is an analysis of that turn by an *in situ* ethnomethodologist. Thus Heritage (1984, p. 256)

> The important thing is that, once again, the interpretations embedded in these treatments of the prior turn are publicly available as the means by which previous speakers can determine how they were understood. Thus the sequential 'next positioned' linkage between any two actions can be a critical resource by which the first speaker (and, of course, 'overhearing' social scientists) can determine the sense which a second made of his or her utterance.

Reflecting on this idea we can of course note that whilst the next turn is, in a very important sense, an analysis of the previous turn does it show an analysis of that turn? Taylor and Cameron treat the next turn analysis idea as the only analytic resource that a student of CA has and point out, correctly, given their premises, that an observer would perpetually be postponing analysis one turn by shifting to the next one, and so on. The damage of this criticism for CA is, I think exaggerated, and in practice things may be less controversial. In his discussion of how the next turn displays an understanding Heritage (1984, 255) gives the following example:

```
 #1
B:  Why don't you come and see me some┌times
A:                                    └I would like to
```

(see appendix 3 for an explanation of the transcription conventions)
We can imagine A's response being:

```
#2
A:  I'm sorry. I've been terribly tied up lately
```

I don't think that it is controversial to say in #1 A interpret's B's turn as an invitation whilst in #2 it is a complaint. Interestingly, Heritage has recently related this issue to Grice's work on conversational implicature (1989, p. 25), and in fact lists Grice, along with Garfinkel and Goffman, as an example of a source of theoretical insight (p. 37). In fact the term "sequential implication" crops up in the CA literature. In a footnote Sacks and Schegloff (1974) say:

> By 'sequential implicativeness' is meant that an utterance projects for the sequentially following turn(s) the relevance of a determinate range of occurrences (be they utterance types, activities, speaker selections, etc.). It thus has sequentially organized implications. (239)

2.7.1 Some principle findings

How does this principle guide analysis? What findings have come out of its application? I propose to answer this by summarising some of the main findings of CA, presenting some findings under four headings, turn taking, preference organisation, topic organisation and the integration of vocal and nonvocal behaviour. Of course what follows is not a summary of the whole field, but rather some illustrative examples which I think bring out different and important points.

<u>turn taking</u>

It is evident from the most casual observation, in western society at least, that conversation is made up of turns at talk being taken by different parties. In their classic paper, Sacks, Schegloff and Jefferson (1974) list fourteen "grossly apparent facts" about turn taking which a model will need to cope with. I've reproduced the list here (omitting the references at the end of each line in the original) since it conveniently sets out the nature of the problem:

(1) Speaker-change recurs, or at least occurs.
(2) Overwhelmingly, one party talks at a time.
(3) Occurrences of more than one speaker at a time are common, but brief.
(4) Transitions (from one turn to the next) with no gap and no overlap are common. Together wiith transitions characterized by slight gap or slight overlap, they make up the vast majority of transitions.
(5) Turn order is not fixed, but varies.
(6) Turn size is not fixed, but varies.
(7) Length of conversation is not specified in advance.
(8) What parties say is not specified in advance.
(9) Relative distribution of turns is not specified in advance.
(10) Number of parties can vary.
(11) Talk can be continuous of discontinuous.
(12) Turn-allocation techniques are obviously used. A current speaker may select a next speaker (as when he addresses a question to another party) or parties may self-select in starting to talk.

(13) Various 'turn construction units' are employed; e.g., turns can be projectedly 'one word long', or they can be sentential in length.
(14) Repair mechanisms exist for dealing with turn-taking errors and violations; e.g., if two parties find themselves talking at the same time, one of them will stop prematurely, thus repairing the trouble.
(p. 700-701)

It is important to bear in mind that this list is intended to give constraints that will apply to models of turn taking. That some of the points clearly sometimes do not hold (for example parties may be told by an operator at the start of certain calls that the telephone conversation will be limited to three minutes) does not remove them from the list of things that a model will need to accommodate.

Sacks et al. go on to propose the "simplest systematics" for the organisation of turn taking consisting of two components and a set of rules. The two components which they have in mind are 1, the "turn-constructional component" which refers to the bits of talk that a speaker uses to make a turn and 2, the "turn-allocational component" which refers to the techniques used to allocate who is to speak next. The latter component is subdivided into two groups, namely where the current speaker selects another or where the current speakers select themselves to speak next. The rules are as follows (p. 704):

> The following seems to be a basic set of rules governing turn construction, providing for the allocation of a next turn to one party, and coordinating transfer so as to minimize gap and overlap.
>
> (1) For any turn, at the initial transition-relevance place of an initial turn-constructional unit:
> (a) If the turn-so-far is so constructed as to involve the use of a 'current speaker selects next' technique, then the party so selected has the right and is obliged to take next turn to speak; no others have such rights or obligations, and transfer occurs at that place.
> (b) If the turn-so-far is so constructed as not to involve the use of a 'current speaker selects next' technique, then self-selection for next speakership may, but need not be instituted; first starter acquires rights to a turn, and transfer occurs at that place.
> (c) If the turn-so-far is so constructed as not to involve the use of a 'current speaker selects next' technique, then current speaker may, but need not continue, unless another self-selects.

> (2) If, at the initial transition-relevance place of an intitial turn-constructional unit, neither 1a nor 1b has operated, and, following the provision of 1c, current speaker has continued, then the rule-seet a-c reapplies at the next transition-relevance place, until transfer is effected.

This may seem like little more than a rather tedious spelling out, spiced or soured with jargon, depending on your taste, of a rather unremarkable observation. Namely that in a conversation there are points when the person speaking continues or hands over to somebody and that at such points they nominate someone else to speak, pick themselves to carry on speaking or do neither of these in which case, if they don't just carry on speaking themselves, then whoever starts first wins the floor. Of course there's nothing wrong with stating the minimum that needs to be said but I do not think that such a summary does justice to the formulation. Because, though capable of fitting with intuitions, the proposed mechanism is non-trivial in that it is not obvious. Levinson illustrates this by contrasting it with what else but other models of turn-taking namely "signalling" models, which propose a signal analogous to the use of "over" in two-way radio communication, and with models which presuppose units beneath the surface on the conversation (p. 302-303). Against the former model Levinson remarks that visual cues can't be fundamental since telephone calls apparently take place in the absence of special intonational patterning and that anyway such signals cannot account for overlap. This point is also Levinson's argument against the ideational model. On this account recipients retrieve the type of move being performed by speakers' surface utterances and then make their own move. Levinson claims that such an account cannot be correct because predictable expressions such as stereotypical greetings, would be subject to more overlap than they apparently are. Although I don't want to get into a discussion of these points it seems to me that what these models would require to improve them would involve attention to turn taking as a locally managed phenomenon which is precisely the point of the approach of Sack's et al..

I want to make some remarks on what they refer to as the "context-free" and "context-sensitive" nature of the organisation of turn-taking. Clearly whatever mechanism there is to turn taking is extraordinarily context sensitive since

speakers need to respond to highly local details moment by moment in different conversations in different times and places. That the putative mechanism holds in such diverse situations is its context-free character.

adjacency pairs

An adjacency pair is, roughly speaking, a member of a class of pairs of turns such as question/answer, greeting/greeting. A feature of the turn-taking model is that it bridges two different turns, which may be made by different speakers. The sense in which it does this is geared to who speaks, rather than to what they speak about. An adjacency pair is the application of the idea that an utterance sets up constraints on what can happen next, or on what some utterance implies, not only to who is to speak next, but to what they are to say. Sacks and Schegloff (1974) talk of the first pair part of an adjacency pair providing a speaker specifically concerned with the "close order sequential implicativeness of an utterance" the chance to achieve it.

topic organisation

Topic organisation attempts to study details of how participants establish certain topics or how they move on to others. Atkinson and Heritage (1984, p. 165-166) remark that though this might appear to be a natural area for CA to study progress has been slow and cautious. I presume that whilst the establishment of a topic is achieved on a turn by turn basis, even if in a step-wise way, a topic is not a conversational object in the sense that a turn or even an adjacency pair is.

Extension of methods to non-vocal behaviour

Conversation analytic techniques have been applied to non-vocal behaviour in communication in a way that contrasts markedly with the approach which enjoyed vogue in psychology during the 1970s. The expression "non-vocal" is, I think, preferable to "non-verbal" when it comes to studying phenomena such as body movement and gaze, since the distinction between vocal and non-vocal phenomena is parallel to that which can and can't be captured on an audiotape. A verbal/non-verbal distinction suggests a subdivision of the vocal into separate phenomena implying a distinction between words and in-breaths, laughter and

many familiar back-channel responses (mmhmm's and so on). Apart from the fact that such a distinction becomes a difficult one to draw, the development of CA happens to have integrated verbal and non-verbal vocal phenomena prior to attention to body movement and gaze.

The strategy of CA treatment of videotape data differs from other approaches in a number of respects. In particular, the structure of the interaction is the focus of interest, rather than frequency analysis of isolated behaviours. And the integration of speech and nonverbal behaviour has been emphasised. Goodwin, for example, in a paper which I discuss in more detail chapter seven (Goodwin, 1987, p. 117), invokes nonverbal behaviour to clarify what a speaker is doing at a point in speech.

```
G.86:490

Mike:   I was watching Johnny Carson one night
        en there was a guy
        by the na- What was that guy's name.
                  ↑
        Mike Shifts Gaze To Phyllis
```

Goodwin, whose attention is on the impact of Mike's forgetting on the interaction is able to draw on other analyses of gaze and word-searches to show that Mike is addressing the question to Phyllis. An important theme of Goodwin's work which is presumably made possible by attention to non-vocal features of interactions is attention to social organisation not at the level of turns - the hallmark of CA - but within turns too. Goodwin (1979, 1981) shows evidence that restarts and hesitations may be understood as devices for securing the gaze of a recepient. In other words the production of a turn may take account of not merely the sequential environment in which it is placed but also concurrent details of the situation.

3.0 Critical remarks on the role of convention in communication

Before moving on to consider how to conceptualize constraints in communication I want to consider a point of tension which emerged in the discussion of different approaches to analysing talk.

Davidson (1984a, 1984b) questions the appeal often made in accounts of communication to the place of convention. For example, is it by virtue of convention that a speech act has the force that it has? When commenting on Austin I remarked that his starting point was with sentences which grammatically are indicative expressions but which do not constate anything. This distinction between the grammatical form of a sentence and the use of to which an utterance is put is the distinction, in philosophical tools of trade, between mood and force. The moods, imperatives, interrogatives, optatives are grammatical classifications of sentences. "There goes Charlie.", "When is dinner?" and "I wish I was in Vienna.", like those unskilled in diplomacy, show their moods. The force of a proposition, at first sight, might be thought to be related to mood. For example assertions seems to be close cousins of indicatives, "There goes Charlie." looks like as good an example of an assertion, an utterance with assertoric force, as one could hope for. But the use to which an utterance is put depends on things beyond the expression used to make it. This is not restricted to Austin's examples such as "I bet you sixpence." but is something more general. On certain occasions an indicative sentence can be used to issue a command "In this house we remove our shoes", or a sentence in the imperative mood can be used to ask a question "Tell me who won the third race" .

Davidson points out that it cannot be by convention that an utterance has the force that it has. He points out, for example, that an assertion cannot label itself an assertion. If there was an assertion sign in language it would of course be used by every trickster. Davidson considers the example of a play during which a house catches fire and an actor has the job of trying to peruade other peole to the best of his abilty of the danger. What if a real fire broke out in the theatre at just that time? What extra resources could the actor have?

4.0 Communicative constraints

The idea that use of the term "constraint" to express pressure that somehow operates in communication is a widely drawn on, but seldom indexed. Perhaps it is felt to be too general a term to fashion into a technical one. I mentioned above that the phenomenon of code switching gives a clear example of an aspect of someone's communicative conduct being influenced by the speech-situation. Gumperz talks of constraints at various points. In the case of code switching we have the flavour of something normative, to do with culturally acceptable conduct taken into the sphere of communication. Introducing the Norwegian study in editorial capacity Gumperz and Hymes (1972, p. 407) say: "... the outcome is an understanding of social constraints and linguistic rules as parts of a communicative system." Gumperz and Blom (1972, p.416) talk of "selection constraints" giving the Ranamål-Bokmål switch a distinctive character, differing from the situation in New York, which, apparently, is similar phonologically. In the conclusion they write:

> Our analysis in this chapter is based on the assumption that regularities in behavior can be analyzed as generated from a series of individual choices made under specifiable constraints and incentives (Barth 1966). This position implies an important break with previous approaches to social structure and to language and society. Behavioral regularities are no longer regarded as reflections of independently measurable social norms; on the contrary, these norms are themselves seen as communicative behavior. They are what Goffman (1959) calls the rules of impression management or, in our terms, in the social meanings which constrain the actor's adoption of behavioral strategies in particular situations. (p. 432)

Elsewhere, however, Gumperz draws on the idea of constraints within code switching itself (1982, p. 86, 89). The point is that switches in code do not occur randomly. He reports some data drawing chiefly on members' intuitions of what would sound acceptable. This phenomenon, though undoubtedly interesting, is separate from my concerns.

In my introduction to this chapter I presented the idea that it is meaningful to talk of constraints in the context of a question-answer sequence. In a paper

entitled "Conversation" Searle mounts an attack on the possibility of giving an account of conversational structure. His starting point is to remark that speech act theory takes single utterances as its subject matter. As I discussed in chapter one, he proposed conditions, in the form of 'constitutive rules' for speech acts to come off successfully. Searle asks if we could get an account of conversation in terms of constitutive rules. His answer is that we can't. To reach this answer he sets off by remarking that some speech acts do have appropriate responses to them, so we have the germ of a sequential analysis. He used the term "constraint" to express this, pointing out, as I did earlier, that for an response to be an answer to a question it is constrained.

> If we consider question and answer sequences, we find that there are very tight sets of constraints on what constitutes an ideally appropriate answer, given a particular question. Indeed the constraints are so tight that the semantic structure of the question determines and matches the semantic structure of an ideally appropriate answer. If, for example, I ask you a yes/no question, then your answer, if it's an answer to the question, has to count either as an affirmation or a denial of the propositional content presented in the original question.

Searle says that in addition to questions requests to perform speech acts also "constrain the form of a possible reply". A third case of constraint establishing speech acts appear to be bets, or offers. Concluding that speech act theory does not get far in producing sequencing rules, he canvasses Gricean pragmatics. He wants to talk of the maxims of quantity and relevance as "external constraints" on speech acts relating to cooperation and relevance. He points out that it is not a constitutive rule of making a statement that it should be relevant. This is true, and it should be remembered that it is a feature of speech act theory that it wants to hunt down constitutive rules.

Workers in the field of conversation analysis have often drawn on the idea of constraints. Examples of this can be found with respect to work on 1, turn-taking, 2, adjacency pairs, 3, preference organisation and 4, topic organisation.

Sacks, Schegloff and Jefferson (1974) (p. 696) say that "...an investigator interested in some sort of activity that is organized by a turn-taking system will want to determine how the sort of activity investigated or is adapted to, or

constrained by, the particular form of the turn-taking system which operates on it."

In his exposition of CA Heritage (1984, p. 260) states:

> We started by describing very tightly organized sequences - adjacency pairs - in which the options available to second speakers were relatively constrained. By contrast, we have now lapsed into a looser mode of exposition in which the local positioning of utterances 'next' to one another is crucial.
>
> We need to work in this way because, as was previously stressed, conversation is not an endless series on interlocking adjacency pairs in which sharply constrained options confront the speaker. Rather conversation is informed by the general assumption - common to both speakers and hearers that utterances which are placed immediately next to some prior are to be understood as produced in response to or, more loosely in relation to the prior.

In her analysis of responses to compliments Pomerantz identifies competing pressures on the recipient. Put crudely the dilemma is to accept the compliment but thereby engage in self praise or to deny the praise but thereby disagree with the speaker's assessment. Pomerantz formulates this in terms of "constraint systems" (p. 106):

> The productions of compliment responses are sensitive to the co-operation of multiple constraint systems. One preference system is that of *supportive* Action$_2$s, that is responses which legitimize, ratify, affirm, and so on, prior compliments. A second constraint system is that of the *self-praise avoidance*.

In their discussion of "topic intial elicitors" Button and Casey (1984) draw on the idea of a constraint to formulate their operation (p. 169):

> Topic initial elicitors then, produce a constraint system, with two options, for the next turn, and within which the production of one option over which the other is preferred. Further the production of either of the options can itself have related sequential consequences for the shape of the following turn.

I use the term "communicative constraints" since I take it that such constraints are intimately part of communication. "Constraints on communication" would not really capture this but would mean obstacles to communication exclusively.

4.0 Conclusion

There are fundamental differences between the methods which I examined. The approaches stemming from social psychology are oriented to establishing statistical relations between events. This approach is not suitable for my purposes because it does not enable conversation structure to come into view. In addition it is open to methodological objections concerning the validity of the use of categorial methods. This is a general problem affecting approaches which rely on the isolation of conversation units of any kind. Are the units to be identified etically or emically? Does conversation require that participants recognise units? This is therefore a criticism to which the Birmingham exchange structure is vulnerable. The ethnography of speaking demonstrates some interesting and clear ways in which aspects of speech, in particular choice of code by bilingual speakers relates to conversational context. The two philosophical theories considered are concerned with a conceptual analysis of certain features of talk. Speech act theory, concerned with establishing rules which determine the successful performance of a speech act does not concern itself with speech phenomena beyond single utterances. Grice's interest moves close to my own in his attempt to describe general features of talk which constrain what people can say. Nevertheless, this analysis of the logic of conversation is pitched at the wrong level when it comes to dealing with empirical data for it does not show how to reveal the conversational logic to be found there. I have suggested that conversation analysis can be seen in this light: an attempt to establish the methods used in structuring conversation.

For practical and conceptual reasons I have chosen to proceed in the manner of conversation analysis. This approach is oriented to revealing the communicative practices of participants in conversation, rather than projecting a model onto their behaviour. This situation is a counterpart of the conclusion that I came to in connection with the parent discipline of ethnomethodology. Although notions of rule following are appealing the truth is that we have no mandate to assume that conversational activity is rule governed. Certainly the hegemony of one application of an appeal to rules in connection with one area of language,

transformational grammar, should in no way imply that this model should be applied to conversation.

Furthermore, CA has proceeded using just the material that participants have available to each other which means that it does not assume that participants need to identify an utterance as belonging to a certain class in order to deal with it, and therefore does not engage in speculation about a taxonomy of utterances. Rather features of the data itself are appealed to.

There are however, limitations to the approach. Clearly native communicative skills are required on behalf of the analyst. I don't see how this could be otherwise but it should not be claimed that research of this kind is entirely free from unarticulated everyday understandings on the part of the analyst.

In a way the imperative of CA is an aspect of a directive which I argued for in chapter two: if you want to study personality terms then look at them in their natural habitat, so too, if you want to study the logic of conversations, look at the practical reasoning constitutive of talking to each other.

CHAPTER 5: EXPLORATION OF COMMUNICATIVE CONSTRAINTS IN ADVISORY EXCHANGES

In this chapter I carry out an analysis of interactions in a computer advisory centre. The analysis is based on the approach of conversation analysis, described in the last chapter, but is limited to those methods which are required to further my claim that talk occurs under communicative constraints. The analysis bears on the topic of explanation in so far as user and advisor are required to explain problem and solution respectively. The situation is however, more interesting and more complex than this. I show that such explanation stages are not discrete episodes at all but are interactively attained through conversation. Furthermore, I show that the appearance of accounts, against the conclusions of some students of explanation, occur as a routine part of the business-at-hand, unconnected with accounting for untoward goings on. In addition to addressing explanation giving itself I also find examples of how a speaker's formulation of some business changes in the course of the interaction. I offer this as evidence of communicative constraints on a speaker's formulation.

1.0 Introduction

Having argued in favour of conversation analysis with respect to both its conceptualisation of conversation and its research methods a natural step would be to return to the data discussed in chapters two and three in an attempt to elucidate how the formulations which speakers produce are a product of the situation in which they occur. This is not the strategy which I will adopt here, not least because it is beyond the scope of my ability and of this phase of my research to apply conversation analysis in such a focussed way. Therefore I have chosen to take a context in which explanations, in the form of episodes rather than utterances, take place to try to clarify some of the ways in which speakers' contributions are constrained by the situation. In this way I hope to offer concrete examples of what has so far here been an abstract claim. My method is based on conversation analysis in so far as 1, I shall avoid a theory based classification of speakers' contributions; 2, analysis will centre on repeated readings of detailed transcripts and 3, I will be interested in inductively

recurrent patterns in the data. Although based on CA the studies presented below should not be understood as demonstrations of this approach, but rather as an exploration of certain details through an attempt to apply some of the points which conversation analysts have emphasised.

The data come from the programming advisory service at the University of Leeds which is a help facility provided by staff of the University's Computing Services Department. Computer users, usually members of the university's academic staff or research students, can consult the advisory, as it is usually called, for help with their computing requirements which vary from detailed problems with writing programs to registering for a computing course or consulting a manual. The advisory is staffed during office hours by one of the members of staff of the computing service on a rota basis. I have followed the indigenous practice of calling someone consulting the service a "user" (from computer user) and I have called the person on duty the "adviser".

The advisory is an attractive place to collect data on interaction because 1, the interactions are short so several examples of sessions being opened and closed can be collected in a given time; 2, the interactions are usually between just two people (which simplifies transcription and further analysis) and 3, the participants are engaged in a non-sensitive task in a location which is convenient to record in. I suspected that these interactions would be particularly interesting in that the participants have to negotiate different knowledge in the situation. Although I do have some examples of this happening the most exciting findings lie elsewhere. My findings fall into two areas. Firstly, issues relating to explanation, namely how problems are formulated and how speakers tend to account for claims that they make. Secondly, body posture during telephone calls. This was an unplanned area of study which I report on briefly in chapter six.

1.1 Relationship of the study to earlier chapters

I have argued above that everyday psychological concepts and causal explanations surface not in a vacuum but in the course of talk where such expressions do something. The method of analysis has followed two suggestions from ethnomethodological conversation analysis. Firstly, the creation of transcripts designed to show the structure of the interaction by paying attention to timings and overlap. Secondly the idea that a hearer's response is an in situ analysis of the speaker's last utterance. As I mentioned in chapter four it seems to me that the claim that the next position in a sequence "shows an analysis" which can be read off by an observer has been over stated and is susceptible to the criticisms made by Taylor and Cameron. Nevertheless I think that attention to what strikes the analyst as important for the hearer is an important resource that a nonparticipant should attend to.

1.2 Earlier work on computer advisory sessions

Once my data collection was underway my attention was drawn to earlier work on computer advisory sessions carried out by computing personnel and published in the human computer interaction literature. I am grateful to Dr Jon Duke for making me aware of this work. The work in question was carried out by J.L. Alty and M.J. Coombs (Alty and Coombs, 1980; Coombs and Alty 1980, 1984). Although the aims of my research are almost diametrically opposed to theirs I feel that it is important to acknowledge it.

Whereas their perspective is one of monitoring and improving the service for me it is an example of a naturally occuring interaction with its own integrity.

In representing my interests to the advisors and users that I studied I distanced myself from having any concern with making judgements of their abilility. Coombs and Alty (1985), on the other hand discuss "potential and actual communication difficulties" and present "long rhetorical sequences during which the advisor externalised his thoughts" as an example. Their point is that such episodes are problem solving rather than educational and were often not understood. We are not told how it is known that these episodes were not understood. I would not feel inclined to analyse multiple - turn episodes in this way.

An additional difference is that Alty and Coombs (1980) are concerned with establishing findings about the usage of university computer advisories and also with such things as user's satisfaction with the service and other characterisations such as differences between experienced and inexperienced users.

Coombs and Alty (1985) report an analysis of advisory sessions using the Birmingham exchange model. They found that they could apply the model to tape recordings of advisory sessions with only a little modification to the original definitions. They present the hierarchical model that they arrived at and include three examples of analysed transcripts. They conclude claiming that the analysis has had important applications in identifying communication failure, development of better advisory and teaching strategies. They do not describe how these applications are achieved and it is not clear to me how the descriptive analysis leads to such applications. It seems to me that the conclusions reached could have been obtained by simply reading and reflecting on the transcripts. But if the coding procedure helped the analyst to get to grips with the the data in a practical way then it clearly has some value.

1.3 Practical details

I wrote to the head of computing services explaining my plans. My request to make video recordings in their advisory was discussed at one of their departmental meetings and was accepted, upon the condition of consent from user and advisor. The departmental assistant circulated a memo round the staff and provided me with a rota showing the advisors who were prepared to be video recorded. This enabled me to turn up for the duration of their advisory session, set up my equipment and ask users as they turned up whether or not they would let me record. Only two users declined out of twenty nine approached. The correspondence with the computing department, and the note which I handed to the users are included in appendices 4 and 5. This should indicate the way I presented my interests to the participants. Whether or not people's behaviour is changed through being video recorded has been much discussed. It has been argued that people still have to communicate and presumably certain formal features of interaction will occur regardless of the participants being self conscious. I do not feel inclined to adopt this approach because I am not sure that formal processes can be separated out from the stream of behaviour. I would argue that the task that the people perform under video conditions is itself interesting enough. Surreptitious recording would not have been practical. I consider this an unecessary and unethical option. Several references do occur to the process of recording in the interactions. At several points my talk with users as they arrive is audible.

The usage of the advisory varies greatly; both in terms of the contents of the consultation and their length, but also in frequency. Although I attended on more occasions I only came away with recordings on eight. I decided to allow the advisor one consultation before recording on each occasion. In total I collected three hours eleven minutes of video tape. Plus an additional 20 minutes on an audio backup tape. Considering just the video tape data, this was from twenty five advisory sessions. Three of these were nested within longer sessions. The quality of the recording varied a lot. It's difficult to talk in terms of an "acceptable quality" since much depends on the hearer's determination! However in some sessions the users spoke too softly to be audible. One

disadvantage of recording in the advisory is that the user access area to which it is connected is quite noisy, including two line printers just outside the door of the advisory. In addition, the advisor's desk has a window behind it, so I was shooting into the light.

I viewed the data several times noting gross features of the interactions. An analysis of body movement during telephone calls, which I report in chapter six, comes out of this process. I made audio copies of the video soundtrack and made transcriptions from selected sessions. I feel that a transcript of the speech is the most useful way to structure the data. Even where my interest was on movement and posture I found the most practical way of proceeding was to mark movement on a transcript of speech. An audio tape recorder is easier to get access to, enables you to concentrate on the sound alone and is easier to use. Some stretches of talk require several hearings before the meaning can be made out. This is an interesting phenomenon in its own right.

The method of transcription was devised by Jefferson. I'm indebted to Dr Paul Drew of the University of York for giving me practical help in using this method. I've followed and developed Goodwin's modifications to the system and extending it to non vocal phenomena such as gaze and body movement. Potter and Wetherell (1987) estimate that the Jefferson system requires twenty units of time for transcribing one unit of tape. The method of transcription is radically different to the method used in the earlier studies. Appendix 3 describes the conventions. I began work on the clearest recordings first: the analysis reported below is based on transcriptions of twenty sessions, about 50,000 words. Some summary details of these sessions make up appendix 5.

All the video taping was done with a tripod mounted Sony HVC 3000P video camera feeding a Canon VR 10 video recorder. A Sony Professional cassette recorder was introduced towards the end of the study. Recording spanned five weeks, being interrupted by the University Easter vacation.

2.0 Analysis

Talk in the advisory may be expected to be a distinctive kind of conversation, different from say, casual conversation, in the following way: The advisory exists, as its name implies and instructs people going there, to enable people to get help with computer-relevant activities. Explanations should thus be a focus of the interactions since if they are to receive advice relevant to their concerns users have to explain what those concerns are and the advisory has to explain some course of action or offer some information which counts as an explanation of the user's concerns. If the business of the advisory sessions is explanation then this should provide opportunity to study those conversational activities which are constitutive of, and carried out in connection with, explanation. In short, how explanations (perhaps of a certain kind) are achieved.

In what follows I want to present several issues which recur in the data. This can hardly be an exhaustive account of all that a conversation analytic perspective can say about this situation. Indeed I do not attempt to give an account of these advice sessions as distinctive, integral species of interaction. Furthermore although I suspect that there is hardly an utterance in my data for which a previous CA finding was irrelevant, I shall not attempt to tell a story simply by applying such finding to the data step by step. The familiar way of proceeding in CA has been the search for phenomena which happen repeatedly in a corpus of data, that is by induction. Schegloff (1987), nevertheless, offers an example of an analysis of a single episode which proceeds by bringing CA findings to bear on it. As even my modest data set will show, a given stretch of talk can reoccur under different analytical considerations. I am sceptical, therefore, that there could be such a thing (at least at present) as a definitive analysis of a situation by the application of CA findings. But as I also hope to show, the findings reported in the CA literature can be applied to specific cases and certainly can direct and illuminate a reading of the data.

Although an a priori model of what is to be expected in these advisory sessions could be constructed, for example by extending my remarks above on the user's requirements to what has to happen for an explanation to come off, I will avoid

doing this. Although I could have a go at producing an account by drawing on my own use of the advisory, my own understanding of the kind of requirements met by computer users at Leeds, on conversations with the computing staff and on impressions made through viewing my recordings and reading the transcripts of them, I shall not do that here. Such an ethnographic approach certainly has value, but my focus is simply on the conversational detail of explanation, details which I have demonstrated to myself can only very exceptionally be noted and recalled without the aid of transcripts. So I have tried to remain close to the transcripts. Though an ethnographic method would not capture member's methods at the level of conversational detail and would therefore not explicate members' practices in putting explanations together I have inevitably drawn on an understanding of the running of universities, the management of computing resourse and a limitless list of other details to make sense of what's going on. Indeed in presenting fragments of transcripts I have to ask myself whether or not there are features that require a gloss to enable readers to get my understanding of the fragment. Of course my own understanding of these features is limited. In one of the sessions which I report where I was making an enquiry the advisor addressed my understanding of computer resources:

```
[UAA.2]
U:    well I mean can I set up my own permanent disks.
            (0.6) .
A:    No you ca- well (.) not permanent no.
U:    [[(uh:-)
A:    [[The whole reason why we have the filestore. (0.6) is (.)
      >that when you set up a permanent disk you're  allocate
      piece- er-< area of disk
            (.)
U:    y:eh.
A:    which only you can use
U:    ye:s
A:    'f y'don't have any files on it.
      (.)
U:    yes=
A:    =it is still in use.=
U:    =right yeh (.) this is-
A:    and therefore (1.0) uh (1.0) (y'know) it can be wasted so
      the whole idea of >the whole idea of the filestore< was
      th'you GeT at the disk space when you need it
U:    yeah
```

In order to offer a context for the fragments which I'll report and to make some comments on the limitations of the data I want to start with some remarks on the recognition of the situation as an advisory session.

2.0.1 Remarks on being-in-an-advisory situation

I don't think a general account of how a cuboid a few metres above the earth's surface in a west European conurbation gets recognised as a place where advice on computing matters can be given here. The data however do show some things about this issue which it is easy to let slip as unremarkable. Firstly the advisor routinely treats the presence of a person in the room as the presence of a user of the advisory and orients to the institutional arrangements constitutive of the advisory. Consider the following exchange:

```
[UAA21i]
((a session is in progress when the door opens and closes, timed
from loudest sound (presumably the door closing) and a woman
comes in))
          (4.7)
A:    We are actually shut now
      (0.5)
U:    Oh I'm so:rry:=
A:    =it's okay >are you j- is it to borr-< borrow a book or
      s[ometh[ing]
U:     [°hh  [I j]ust wanted to have a [look in a manual is]
A:                                     [ yeh  go  ahead   ]
U:    that alright
A:    ye[h  okay ]
U:      [thankyou]  ghuh ghum ((clears throat))
```

This fragment, the shortest one that I recorded, reveals a considerable ("awesome" as Garfinkel might say) competence on behalf of the participants. Here I just want to note that entering the room is oriented to as the entry of a user, a user who has come at a time when normal user activities are somehow problematic. In addition the advisor treats the user's response as the response of someone who has been rebuked (He says that "It's okay" and he finds a user relevant activity which would make the user's presence unproblematic). Indeed he gives the go ahead for the user to fulfil her requirements before she has finished her request, a request which he nevertheless acknowledges.

Reciprocally the advisor needs to be recognised as a source of help for a session to happen. Here a session is in progess when (U) comes in and rests a manual on the advisor's desk to consult something. (U) has been there a while at the point where the following fragment starts.

```
[UAA.9]
A:    well while that's doing, I- I'll see (who's) waiting
      (0.8) what you you've got a a- you think you've got a
      problem with er-=
U:    =yes [tha::]
A:         [ghost] have you
U:    mm: (.) this w- trivial one and then I wan' to ask
      something about using some the (.) nag routines wither:::
      intregral °analysis °
```

(A) lifts the manual up to look at it, and is hence able to guess the user's problem. This is a thorough identification of the man leaning on the desk as a user, someone to whom help can be offered and who's activities are interruptable to this end. (The "Ghost" is a graphics printer, a "nag routine" is a ready made computer program which can be called from a library of such programs, "integral analysis" refers to "numerical integration", a computational procedure for carrying out a certain mathematical operation.) In his reply it is imaginable that the user might say "hand's off what's it got to do with you" but the user knows that this man is an advisor, to whom not only an affirmative response is given but to whom another requirement should be lodged.

Identifying the advisor is not automatically unproblematic. On two occasions that I recall I was asked for help by users. On one occasion I can be heard off-camera talking to a user who, following my request that I video record his consultation, proceeds to address his problem to me. Interestingly users sometimes down-graded the interest that their session might have for me. For example (off-camera talk):

```
[UAA.18]
U1:  s'only going to be a brief one
```

In the following four sections I will explore four different things that strike me as interesting about the data. Firstly, how speakers may complete an expression

started by the other person; secondly, that speakers change the way that an object or activity gets formulated during the interaction; thirdly the giving of accounts. Finally I will make some remarks on four, the user's initial expession of their requirements and the advisor's initial formulation of a response.

2.1 Collaborative completion

Paul Drew has pointed out to me that in the following section there are cases where a recipient completes a turn which is somehow problematic. The arrows point to such completions. In these cases the speaker whose turn gets completed continues in a way which accepts the completion.

```
[UAA.20] #1.1
U:    all I want to (do:)  is h:ow I can: (1.5) use
                   (know)
      kermit on the (1.3) pee see five dw'I need any
      additional: chips or:: anything or ┌(0.7) will┌ the
                                         └no       └ no
U:    software just=
A: a→ =It'd just be the software:
           (0.8)
U:    (Th') kermit softwa:re (0.6) will do it.
A:    Tha's right yeh I mean y- y- you've got to have th the
      pee see:z:  got to be plugged into a um=
U: b→ =a ser- a ┌ a line=
                └ a line
A:    =obviously a line yeh ┌ um
U:                          └ so who will tell me what
      configuration for the (0.5) er::=
A: c→ =f'th'kermit side of i┌t
U:                          └ yeh=
A:    =oh (0.5) em (0.3) w┌ell
U:                        └ I just wan' y'know obviously I'm
      close to a a em nest of terminals with tack lines
```

Drew has suggested the name "collaborative completion" for such phenomena and has isolated cases of which the following are examples (Drew, 1979):

```
(I)  [Sch:I:14:15]
Jill:      I- was thinking of trying to make one. °hh
           Just getting a- a:::, (1.0) uh, a tubing of
           some sort and pu- a::nd°hh
Ellen: →   Wi:ring it,
Jill:      And wiring it, and painting it brow:n or
           black. Can't you just get a piece of pipe?
           and ┌paint it?
Ellen:      └Certainly.
```

Ben: Yea:h, you, uh, you ordinarily use thin pipe,
Jill: Yeah that's what I would want ve:ry thin.
Like uh::
Ben: → Quarter inch.
Jill: Quarter inch, or ha- or half in[ch.
Ben: [No half is
too big. Well it'd be alright,
Jill: It'd be about this big around,

(II) [NB:IV:2:R]
Gladys: ...tacos fer the children w'n they come down
dimo:rrow? °hhhhh a:nd uh,h (.) sh- an' I,m
gontuh phone the orduh tuh the gro:cuh this
morning cz I hù we won(.)'t get
u[p th°e-:r]e
Emma: [°N o : .°]
Gladys: °hhhhh So è-d'you need uh hhamburger don't ↓chu.
Emma: °hh Ye:u::s? e[n y]uh need[some: u]h :]
Gladys: → [E n] [s- °hh]sh:r]edded
lettuce?
Emma: shredded lettuce en CHEE::SE?
(0.4)
Gladys: Oh:, any (.) s- puhticuluh khha:nd?

(III)
Caller: Who am I talking with.=
Wendi: =°hhhh This is Wendi the receptionist?
Caller: Linda the receptionist. Linda?hh this is=
=((click click))
Wendi: Hello?

- - - - tape cuts - - - -

Wendi: Good mo(h)rni(h)ng Bre:tts?
Caller: Linda the receptionist just goofed.
Wendi: Uh no: I didn't. I was sitting here ans a:ll
of a sudden it jus::t
(.)
Caller: → went to a dial to:ne.
Wendi: Ye:ah hu[hh
Caller: [Ye:h that's what it did here too

Each of these sequences appears to have the following three turn structure: a first part which I'll call a "completable" which show some disruption, the recipient provides a "completion" and the original speaker then continues with a "ratification", echoing the completion (wiring it/And wiring it), (Quarter inch/Quarter inch), (shredded lettuce/Shredded lettuce) or acknowledging it (went to a dial tone/yeah huhh). Only (c→) above has precisely this form. I will return to (a→) and (b→) later.

```
[UAA.20] #1.2
A:     =obviously a line yeh ┌ um
U:                           └ so who will tell me what
   1→ configuration for the (0.5) er::=
A: 2→ =f'th'kermit side of i┌t
U: 3→                       └ yeh=
```

This structure, [completable][completion][ratification] occurs several times in the data in just this form:

```
[UAA.9] #1.3
U:     this one is er: which is er trivial is. (.) I want to
       plot some bi:g picture on the calcom plot or
       °aychpeeplot°=
A:     °(big) picture°=
U:     =mm but is thuh (.) the mapping (.) which is er zero to
   1→ one zero to one you've gotta define a (.) sch:::
A: 2→ spa┌ce  first ┐
U: 3→    └mm s'right┘
              (.)
U:     it's zero to one and zero to one which gives you (.) the
       square page.

[UAA.9] #1.4
A:     =well you can do a call pee space (0.8) er=
U:     =z:ero to one. zero to one is the maximum (0.7) for the
       system:?=
A:     yeh (.) well (.) that's right so you can do thee ex (.)
   1→ you can do zero to:
U: 2→ whadever you wannoo=
A: 3→ I believe er- whatever you want s'you can do zero to five
       or something zero to one (1.4) the wy you can't- in- the
       wy can't be any greater than one b'cos that's the width
       of the pa┌per but┐ the- the x is the length of the paper
U:               └m paper┘
A:     so that can be (.)
U:     okay

[UAA.9] #1.5
U:     do can it- can it do: one thing (0.4) can it do this bit
       first.  phi: (1.3) one at a ti::me (.) so will do this
       bit first (.) and then go for thee second one
              (.)
?:     °maybe°
              (2.8)
U:     can it can it do this integration part first
              (.)
A:     what with respect to er beta
              (.)
U:     yeha
A:     °well yeh of course.  that's the way you do an
       integration isn't it°
U:     and then (                                    )
```

A: well yeh I mean that's the way you do it I mean I wanted
1→ to see whether there was a (.)
U: 2→ >ready made one<
A: 3→ whether there was a (.) ready ma:de routine you see (0.9)
is funct- function form of integrand known the answer is
n:o:
U: mm hm

[UAA.9] #1.6
A: How many v'these data points have you got.
(.4)
U: ah::m:: (0.8) about twenty hwords in every::
(0.9)
A: °'bout what?°
U: twe:nty=
A: =in er (.) and is the[e um gen]erated a griD.
U: [( )]
(0.9)
U: they:: (.) are:: (.) not perhaps
A: 1→ °(not) ° they're randomly s-
(0.9)
U: 2→ °spa:ced°
A: 3→ randomly spaced even phi beta=even=I'm talking about in
phi beta space=
U: =yeh=
A: =so [y'have]
U: [no no] i- in- in in when it- we've got a phi: is
changing phi:
(0.7)

Before asking what the implications of the occurrence of such objects might be I want to focus on some borderline cases to see what they might reveal.

Sometimes cases are encountered which have the air of collaborative completions but which, on close inspection lack one of the two flanking components which I presented above. That is, we find [completable][completion] and [completion][ratification]

2.1.1 completion without clear ratification

(IV) [DA]
Betty: Look I: spent- I didn'even go tuh see huh
that o:ften becawss uh::°hhhh uh:: even
though uh:: we spoke to one another on the
phone or ruh things like that-°hh I, I-I jis
couldn'take thee constant repetition
1→ of[uh::: [:::
Fanny: 2→ [of- [of the same story. Oh don'[I kno:w=
Betty: [u-

```
Fanny:      =or how enla:rge'it was or why huhr artery wz:
            five times
```

```
[UAA.11] #1.7
U:      erh we're second year,
              (0.4)
A:      yeh,
U:      undergraduate students (0.6) and we >an I've< been
        advised by (.) Mr Roberts of the French department (.) em
        to do a (.) one day. course er two day cour:se
A:      y[eh  ]
U:       [that's] an introduction to the amdahl
              (0.4)
A:      y:es
              (0.6)
U:      an: we were told to come and see you
   1→   to (.) [find ou]t when
A: 2→          [book up]
              (.)
A:      yes well the cou[rses are ] (0.7) there aren't very many
U:                      [times °av]ailable
        courses this year, (2.0) am: (10.6) there are no (.)
        completely basic courses. (1.0) for beginners left this
        year
              (0.4)
```

#1.7 Can be analysed as a collaborative completion. The timing of the advisor's "book up" does imply that the pause in "to (.) find out", although only a subtle perturbation, is heard by him as a completable. Tentatively, I want to suggest that the fact that the speaker has continued beyond what I am taking to be an offered completion does whatever interactional work a ratification would do in this context or (which comes to the same thing from her standpoint) that the speaker is not hearing the advisor's intervening talk as a completion which would probably also entail that she does not acknowledge her pause as a completable. The fact that the speaker continues with the apparently oddly placed "times available" could be evidence for this.

2.1.2 completion without clear completable

```
[UAA.20] #1.8
U:      you can use t'kermit as a terminal terminal emulator?
A:      yes that't right yes I mean indeed you have to use its
        (styulised) facilit[ies
U: 2→                      [ to be able to do it
A: 3→ to be able to use it as a file transfer yeh (0.6) and er
      (0.7) you know its its as well to know about the um (0.7)
      things to avoid doing (                ) when
      w when you're using the pee see  five (0.9) er: (.) in
      order to keep the session going. (.7) but if you like
```

```
          I'll give I'll give Agnes a buzz and er=

[UAA.20] #1.9
A:        what  (.) what we have got that you just reminded me of
          is that (.) we have the bit of paper somewhere over
          there=
U:        =I- I've got the piece o┌f  paper
A: 2→                             └you (.) y'have explaining about
          kermit
U: 3→ yeh
          [[
A:        yeh okay
U:        but it's more that (.) more like (0.4) if I need anything
          extra on this opus (.) er and and ┌obviously I'll need a
A:                                          └ yeh
U:        (0.7) piece of wyer fuh-
```

These deserve to be called collaborative completions since, although there appears nothing to me as an analyst that could be counted as a completable, the recipient treats the speakers turn as a completable or at any rate an interuptable: the warrant for calling it a completable is that the speaker 1, gracefully leaves the floor and 2, produces some sort of ratification that is, acknowledges what was said by recipient to be what they were going to say.

An additional feature of #1.8 is the actual form of the ratification. It consists of an elaboration of the completion, filling out "do it" with "use it as a file transfer". This is nevertheless packaged to close with an affirmative "yeh". Applying such considerations to #1.9 suggests that (A)'s completion could itself be a ratification of an earlier completion by (U). There is some circumstantial evidence for this interpretation: Looking back we find 1, a formulation by (A) which (A) could hardly have intended to stand as an adequate designator of the object referred to (the piece of paper explaining about kermit) and 2, a restart by (U) at the start of turn which is often associated with a floor-winning device. This could lead to the following tentative formulation:

```
[UAA.20] #1.10
A:        what  (.) what we have got that you just reminded me of
  *1→     is that (.) we have the bit of paper somewhere over
          there=
U:*2→     =I- I've got the piece o┌f  paper
A:*3→                             └you (.) y'have explaining about
          kermit
U:        [[yeh
A:        [[yeh okay
```

```
U:    but it's more that (.) more like (0.4) if I need anything
      extra on this opus (.) er and and [obviously I'll need a
A:                                      [yeh
U:    (0.7) piece of wyer fuh-
```

Despite the uncertainty of the possibility of construing *1→, *2→ *3→ as a full-blown collaborative completions, these considerations explain why "I've got the piece of" should be heard by (A) as a completable: (U)'s emerging affirmation cannot be relied on as an affirmation of what (A) was setting out to ask.

2.1.3 application of the analysis

This fragment looks like a paradigm collaborative completion with all three parts present.

```
[UAA.19] #1.11
A: 1→ I- I can (.) I ca[n
U: 2→                 [pass it on=
A: 3→ =I can pass it on to him y:e:ah. (.) c- can I keep this
      bit of print? out.=
U:    =>y:es<
```

Looking at the preceeding turn (see #1.12), however, shows that the dysfluency in 1→ is interactional: both parties speak simultaneously and compete for the floor. At least this is one analysis. I would prefer to maintain that this is a collaborative completion, even though it may appear more competitive than collaborative because primarily, whatever the antecedent of (A)'s dysfluency, (U) does treat 1→ as a completable. A secondary consideration is that (A) responds to (U)'s turn, whatever it is, as a completion, filling it out and adding an affirmative as I noted earlier.

```
[UAA.19] #1.12
((replaces phone))
                (0.6)
A:    No:. He's not there either, (0.6) er:m (1.7) we:ll.
            (0.6)
U:    [[You cou-]
A:    [[I- I can] (.) I ca[n
U:                        [pass it on=
A:    =I can pass it on to him y:e:ah. (.) c- can I keep this
      bit of print? out.=
U:    =>y:es<
```

The cooperative appearance may be reinforced in contrast to the following fragment where the advisor takes over the user's attempt to recap the procedure which they have worked out. Note that the arrowed turn contrasts with the completions presented above 1, it reformulates (U)'s agenda 2, it does not lead to a prompt return of the floor to (U).

```
[UAA.9] #1.13
U:     I think I got the feeling
A:     you you understand what
U:     yes
       (exactly soon) this is the first ste┌p
A: →                                       └it's two steps
       that's the first step that's the integration with respect
       to be:ta
U:     and (                ) the second step
A:     and they both- you'll both use- write down the name of
       this routine. dee oh one jee ay eff you'll both
U:     dee (.) oh (.) one
A:     jee ay eff
```

Earlier in that session there are cases where (U) chimes in during fluent talk from (A) echoing what (A) has said, 1→ 2→ and 6→ in #1.14. This seems to be built to show that (U) is literally following (A)'s advice. 2→ is incidentally I suspect a true collaborative completion. The lengthening of the n of next is an appreciable pause in (A)'s speech. Interestingly (U) shadows and projects what (A) is saying. Presumably having said exactly what (U) projected, no interactional work is necessary on (A)'s behalf to further mark ratification of (U)'s completion.

```
[UAA.9] #1.14
A:     I believe er- whatever you want s'you can do zero to five
       or something zero to one (1.4) the wy you can't- in- the
       y can't be any greater than one b'cos that's the width of
       the pa┌per but┐ the- the x is the length of the paper so
U: 1→      └m paper┘
       that can be (.)
U:     okay
             (.)
A:     any (.) the only problem is you'll the- the the plot will
       go over the onto the n::┌.ext      piece          of┐
U: 2→                          └onto the next piece of paper┘
A:     paper so there'll be wh perforations across the middle of
       the plot so it won't look very nice (0.6) but-
U: 3→ but for my design ┌it's  alright┐
A: 4→                  └you can do it┘ yeh you have pee space
```

```
      greater than [er (greater than)]
U: 5→              [now second      ] thing which I have in
      mind is I have some double integration (.) on (1.8) I've
      got a function (.) er: which is (1.2) er (.) phi (.) beta
      (.) nine. nine so (.) I've g- I've got this I is
      dependent on (1.2) ph:i: (.) is dependent on (.) phi: and
      beat (.) phi
            (1.6)
A:    I is a function of phi and beta phi=
U:    =phi and be[ta
A:               [okay=
U:    now I don't kno:w (.) e- exa:ct (.) relationship between
      (.) I (.) phi and °beta° (0.4)
U:    [[but ] I got the value of phi: with different values
A:    [[( ) ]
U:    of [I:
A:       [so you've got- (0.7) you've got in tabular form in
      other words y'v got a table of functional value[s  ]=
U:                                                   [yes]
A:    =but you haven't got the functional [ru:le    ]
U: 6→                                     [function]
U:    r[ight]
A:     [ ok ]ay=
U:    =what I wanted to integrate is (.) from (1.2) mm: (.)
      phi? is equal to zero to pi by two:,
```

I want to apply some considerations which have developed in looking at collaborative completions to 5→ . This a redirection, as in the example above. I was puzzled by the aggressiveness with which (U) proceeds to the new topic, although the graphics problem was marked as trivial (see #1.3). The answer lies partly in the fact that (U) is not so much competing with the advisor for floor space, but with another user for the advisor's time. This point, the resolution of the first and trivial problem, is likely to be vulnerable as a place where (U) may be displaced. But I think that this is only part of the picture. Take 3→, this looks like a collaborative completion and certainly occupies a slot where one would be appropriate. (Incidentally, "slot" is a term which I am imposing on the data, only in reporting could it be called a "slot", for the participants it is simply the latest thing to be happening). But I doubt that (A) would have said "it won't look very nice but for your design it's alright". On this analysis the next slot after (U), the place which a ratification could occupy, is a place where I would expect (A) to formulate a version of his projected speech, and this is I think what we have. The net result is that (A) rehashes the solution which (U) has already got. (I think that 3→ is a closing down

move which in a sense fails because of its sequential position.) Hence the legitimacy of (U)'s brisk change of topic.

2.1.4 A hypothesis about the completion slot

After many, many listenings to cases such as the following, in order to be able to remove the doubt-parenthesis around "automatically", and thereby enlarge my data set of collaborative completions I had to admit defeat.

```
[UAA.5] #1.15
A:    yes (0.7) you have: er:::m six hundred (0.6) minutes
      (0.6) per week (1.5) so (.) you're allowed to ask for:
      twice that. so you can ask for another six hundred
            (0.7)
U:    yes
            (.)
A:    and that will be (.) granted (.)
U: →  °(aut┌omatically)°┐
A:         └automatically┘
            (0.6)
A:    but if you ask for more than tha- (0.9) then there will
      be a long delay:
```

There are, however, reasons to suspect that there are constraints on the completion slot. If the user is to show understanding before the advisor formulates the expression themselves then they have to do it fast.

```
[UAA.9] #1.16
A:    any (.) the only problem is you'll the- the the plot will
      go over the onto the n::┌ ext      piece          of┐
U: 2→                         └onto the next piece of paper┘
A:    paper so there'll be wh perforations across the middle of
      the plot so it won't look very nice (0.6) but-

[UAA.9] #1.17
A:    well yeh I mean that's the way you do it I mean I wanted
      to see whether there was a (.)
U: →  >ready made one<
A:    whether there was a (.) ready ma:de routine you see (0.9)
      is funct- function form of integrand known the answer is
      n:o:
```

This means that use of delay, which is often associated with dispreferred activities is ruled out, speaking softly is an alternative.

```
[UAA.9] #1.18
A:    How many v'these data points have you got.
            (.4)
U:    ah::m:: (0.8) about twenty hwords in every::
            (0.9)
A:    °'bout what?°
U:    twe:nty=
A:    =in er (.) and is the[e um gen-] er ated a griD.
U:                          [ (    )]
            (0.9)
U:    they:: (.) are:: (.) not perhaps
A:    °(        not) ° they're randomly s-
            (0.9)
U: →  °spa:ced°
A:    randomly spaced even phi beta=even=I'm talking about  in
      phi beta space=
U:    =yeh=
A:    =so [y'have]
U:        [no no ] i- in- in in when it- we've got a phi: is
      changing phi:
            (0.7)
```

2.1.5 Further complex cases

Earlier I reported a case which I suggested, tentatively could support two readings as follows:

```
[UAA.20] #1.19
A:    what .(.) what we have got that you just reminded me of
      is that (.) we have the bit of paper somewhere over
      there=
U: 1→ =I- I've got the piece o[f  paper
A: 2→                        [you (.) y'have explaining about
      kermit
U: 3→ yeh ·
       [[
A:    yeh okay
```

Here (A) collaboratively completes (U)

```
[UAA.20] #1.20
A:    what  (.) what we have got that you just reminded me of
   1→ is that (.) we have the bit of paper somewhere over
      there=
U: 2→ =I- I've got the piece o[f  paper
A: 3→                        [you (.) y'have explaining about
      kermit
U:    yeh
       [[
A:    yeh okay
```

Here it is (U) that completes (A), in other words, to use the jargon advanced earlier, the completable in #1.9 is a completion in #1.20, or to put it another

way round, if both these readings are correct, a completion is itself vulnerable to completion. This is tantamount to saying that the completion offered is not acknowledged as such and that therefore the ratification slot is occupied by an interuption or, if you like, a second order completion. In the following fragment (U)'s "yeh" suggests that (A)'s prior turn is heard as a completion:

```
[UAA.5] #1.21
U:     (       ) after er five it's free::
             (1.1)
A:     after six=
U:     =(after six)=
A:     well now it's (.) its not w-uh huh ((laugh)) if can't log
       on (.) during (0.5) nine to five. (.) >nine to< six (.)
       you will be allowed to log on after six
U:     yeh
A:     but if if you us:e
             (0.7)
U:  → all th┌e (             )┐
A:  →       └ a lot more ag┘ain.
U:  → yeh
A:     then (.) it will not let you log on after si:x (.) but it
       will let you log on after ten
```

If we have a case of a completed completion where could all the parts lie? We could have:

A: [completable]
B: [completable] ≡ [completion]
A: [ratification] ≡ [completion]
B: ≡ [ratification]

```
[UAA.20] #1.21
A:              what  (.) what we have got that you just reminded
                me of
   1→           is that (.) we have the bit of paper somewhere over
                there=
U: 2→ *1→       =I- I've got the piece o┌f  paper
A: 3→ *2→                               └you (.) y'have explaining
                about kermit
U:       *3→    yeh
                [[
A:              yeh okay
```

This however, is not the only structure that can be found in connection with complex collaborative completions. In #1.23 the user offers a completion which the advisor chimes in with. The completion offered by the advisor however is

different to the one that the user offers, but both parties in turn ratify what the other said.

```
[UAA.9] #1.23
A:          ' come along on monday and I'll have a look at it then=
U0:         ='kay thankyou very much indeed=
A:          =o[kay ] (0.4) I'll log you out
U:            [okay]
A: 1→       now (1.0) I know that there's a (0.6) a- er routi:ne
            for:
                (1.0)
U: 2→ *1→   this sort of [thing ]
A:    *2→                [integr]al=
U:    *3→   =yeh=
A: 3→       =>that sort of thing< in: (.) one dimension
            (integra[ls) I'm] not sure whether there is for two
U:           [ mm   ]
            dimensional integrals


[UAA.20] #1.24
U:    all I want to (do:)  is h:ow I can: (1.5) use
                   (know)
      kermit on the (1.3) pee see five dw'I need any
      additional: chips or:: anything or [(0.7) will[ the
                                         [no       [ no
U:    software just=
A: →  =It'd just be the software:
            (0.8)
U:    (Th') kermit softwa:re (0.6) will do it.
A:    Tha's right yeh I mean y- y- you've got to have th the
      pee see:z:  got to be plugged into a um=
U: →  =a ser- a [ a line=
    →           [ a line
A:    =obviously a line yeh [ um
U:                          [ so who will tell me what
      configuration for the (0.5) er::=
A: →  =f'th'kermit side of i[t
U:                          [ yeh=
A:    =oh (0.5) em (0.3) w[ell
U:                        [ I just wan' y'know obviously I'm
      close to a a em nest of terminals with tack lines
```

2.1.6 Conclusion

The phenomenon of collaborative completion is a specific case where the sense in which the advisory sessions are cooperative, jointly constructed, endeavours comes out. The phenomenon makes visible some of the ways in which people understand and orient to what is going on in an interaction in which they are

participating. The next two phenomena which I want to discuss are somewhat less formal.

2.2 Reformulation

In chapter two when discussing the idea of attribution style I suggest that the causal expression that a person comes out with is possibly discursively produced, in a sense the product of that speech situation. This is also a view implicit in Gilbert and Mulkay (1984) idea of repertoires, an idea that Potter and Wetherell (1987) adopt. A primary case of a capacity to manage and produce different explanations, tell different stories about some matter or have different understandings of what's going on in some situation or generally have different formulations of some issue would be the conversational management of different formulations. The achievement of a different formulation of some matter as the result of an argumentative process would be a strong and explicit case of this. In the section that follows I want to pull out different cases of changes in formulations in the advisory session. This has a certain relationship to Garfinkel's study of the documentary method, of "fact production" in flight, seeing how people update their understanding of what is going on. Here I have a naturally occuring situation and I don't have protocols to get at the subject's understanding of what the other party says.

2.2.1 Analytic outline

I want to discuss reformulations under three headings, corresponding to a gross distinction between different changes in the way that something was expressed in my data in terms of who is speaking.

(1) Self reformulation: where a speaker uses an expression at some point and then later adopts a different expression.

(2) Other reformulation: where a speaker uses an expression, but the other speaker uses a different formulation.

(3) Adoption of other reformulation: following (2) first speaker subsequently adopts second speaker's expression. This is a special case of (1).

Diagrammatically:

(1)
A: →[f]
.
.
.
A: →[f*]

(2)
A: → [f]
.
.
.
B: → [f*]

(3)
A: → [f]
.
.
.
B: → [f*]
.
.
.
A: → [f*]

Now a number of conversation analytic findings are relevant to here.

2.2.2.1 repair

Repair, as it is formulated in the classic paper "The preference for self-correction in the organization of repair in conversation." (Schegloff, Jefferson and Sacks, 1977), is distinguished from correction in that correction is taken to be a species of repair. Correction is generally taken to be the replacement of something faulty with something else. This is too narrow to capture cases which Schegloff et al. want to study. For example in a word search the retrieved name does not replace anything faulty.

```
Clacia:      B't a-another one theh wentuh school with me
          →  was a girl na:med uh, (0.7) °W't th' hell wz
          →  er name.° Karen. Right .Karen.
```

At this stage I intend to use the term "reformulation" in a weaker sense still, for the sake of safety. I would not want to defend the occurrence of a synonym, or a more precise expression as a repair, but I would not want to deny that it is a repair. So corrections are a subset of repairs which are a subset of reformulations.

2.2.2.2 formulating place

Schegoff (1972) addresses the issue of how an object, in particular a location can be referred to in different ways (p. 81).

> For any location to which a reference is made, there is a set of terms each of which, by a correspondence test, is a correct way to refer to it. On any actual occasion of use, however, not any member of the set is "right". How is it that on particular

> occasions of use some term from the set is selected and other terms rejected.

Pomerantz and Atkinson (1984) focus on the way in which the damage to a garment is formulated by the plaintiff in a small claims court. In my data it is clear that speakers can, as a matter of course, draw on a range of coreferring expressions. The following example is made up of fragment of all the different way in which the personal computer, an Opus PC 5, gets referred to:

```
[UAA.20] #2.1
OPUS PC5

1
A:     Y┌eh
U:      └ RIGHT(.) what do you know about kermit(h) (.) and
   →   opu┌s (pee see) (.) pee see fives:
A:         └um uh::m
A:     Well I er I kn-(.) what I (.) do know I copyright (.) er

2
U:     (Th') kermit softwa:re (0.6) will do it.
A:     Tha's right yeh I mean y- y- you've got to have th the
   →   pee see:z:  got to be plugged into a um=
U:     =a ser- a ┌a line=
                 └a line

3
U:          all I want to (do:)  is h:ow I can: (1.5) use
                           (know)
   →   kermit on the (1.3) pee see five dw'I need any
       additional: chips or:: anything or ┌(0.7) will┌the
                                          └no       └no
U:     software just=

4
A:     for the (0.5) e::r its R S two three two isn't it
A: →   yeh. ┌ (0.4) I mean we we've actually got a (.) an opus
U:          └yeh
A: →   five (0.7) with ay kermit (0.4) er (.) on it
       ┌(0.6) erm
U:     └and it's had no modifications (to speak of)

5
A:     things to avoid doing (                ) when
   →   w when you're using the pee see  five (0.9) er: (.) in
       order to keep the session going. (.7) but if you like
```

```
6
U:    I had a word with her last week:  y┌ou know but then I
A:                                      └ yes
   →  hadn't got me opus=
A:    =yes okay
U:    so I was sort of using (0.5) this (.) huh (.) system up
      here which is u:seless

7
U:    =I- I've got the piece o┌f  paper
A:                            └you (.) y'have explaining about
      kermit
U:    yeh
       [[
A:    yeh okay
U:    but it's more that (.) more like (0.4) if I need anything
   →  extra on this opus (.) er and and ┌obviously I'll need a
A:                                      └ yeh
U:    (0.7) piece of wyer fuh-

8
A:    (0.5) em (.) I've got a somebody here (.) who (0.8) erm
      would like a few words about (0.5) running kermit on a
   →  pee see five. (0.8) on an opus
            (2.9)

9
U: →  I've got me opus pee cee five now. ┌(0.6) and what I
A:                                       └yeh
      want to know is (.) um (.) h:ow to configure thee (.)
      interface (.) I'm I'm close to a a room full of tack
      lines (.)

10    ((second advisor has entered))
U:    I'm (.) mainly it's just going to be for transferring
      files I mean in in the future I might use it (.) for
      editing with since I've got tack lines there and proper
      terminals (.) makes sense to use um (.) in the
      environment I'm used to (.) rather than fiddling about
      with keys (.) er er
A1:   yes that's the  yeh then in that case there's absolutely
   →  no problem ┌ for you with the pee see five (.) transfer
U:               └ no
   A1:  files in the usual way (.) you know (.) setting
```

It is difficult to see a pattern in the way that these different terms are used, but I would like to note that 1, the full name of the machine is used when a new recipient is addressed ((U) in 1 and 9, and is constructed by (A) in 8) 2, in possession "opus" is always used (4, 6, 9, but see 7); whereas 3, usage or running of the machine is formulated by "the PC 5" (5, 10). The "PC" in (2) refers to personal computers in general. The point that I want to stress here, however,

is that in the course of talking speakers have the competence to manage a range of formulations of some object. I want to take the analysis further by considering the three different possibilities which I identified earlier; self-reformulation, reformulation by another person - in particular the advisor's reformulation of the something the user says and change within a speaker's formulation following a reformulation by another speaker.

2.2.3 Self reformulations

```
[UAA.20] #2.2
1
A:      yes okay well I don't think we'll have any problems (0.4)
        when you're using it as a terminal emulator there a:re
 →      one or two little snaggles that [ we know about (0.6)
U:                                      [hmm
        which kermit (.) which Agnes will t[ell you about
                                           [hmm

2
A:      yes sell [my advice is that you you ought to see Agnes
U:               [(mm)
A: →    before you start anywhere on this because of this slight
   →    keyboard problem that we know about [er with the
                                            [mhm
A:      terminal emulator [but I'll give Agnes a ring because i-
U:                        [right
A:      if she's she won't want to be interupted but em

3
A:      Basically I think he's g- he's go:t it (0.6) er and he
        just wants to know is there anything special he should know
        and I said well (0.6) we (.) we we do know that the
   →    terminal emulator is less than perfect and I thought you
        might be able to (0.1) give him a few tips on how on how
        how to- he wants to use it for file transfer you see
```

Nowhere in the intervening talk did the user make any reference to this issue. Such changes in formulation are to be distinguished from cases where a speaker simply changes their mind about what they are doing, rather than changes the way that they formulate it:

```
[UAA.8i] #2.3
A:      right (.) I'll do it in a second we'll just em (2.0) I'll
        just we'll do it now: (.) we'll log in one r- one of these
        terminals over here and do it while this chaps putting in
        the data.
```

Some reformulations make reference more specific:

```
[UAA.10] #2.4
A:    =I mean there are various (different venues) around the
university?
              (0.8)
U:    c'z I found the Bayne's wing (0.5) um (0.9) which have got
    → a few word processors in them=
A:    =yeh=
U:  → =bee bee see °micros°
              (0.7)
```

In the following cases reformulations have an explanatory function:

```
[UAA.9] #2.5
U:    now second thing which I have in mind is I
      have some double integration (.) on (1.8) I've got a
      function (.) er: which is (1.2) er (.) phi (.) beta (.)
      nine. nine so (.) I've g- I've got this I is dependent on
      (1.2) ph:i: (.) is dependent on (.) phi: and beat (.) phi
            (1.6)
A:    I is a function of phi and beta phi=
U:    =phi and be┌ta
A:              └okay=
U:    now I don't kno:w (.) e- exa:ct (.) relationship between
      (.) I (.) phi and °beta° (0.4)
U:    [[but ] I got the value of phi: with different values
A:    [[(  )]
U:      of ┌I:
A:  →      └so you've got- (0.7) you've got in tabular form in
    → other words y'v got a table of functional value┌s ┐=
U:                                                   └yes┘
A:    =but you haven't got the functional ┌ru:le    ┐
U:                                        └function┘

[UAA.5] #2.6
A:  → this is how much (.) this is additional
              (3.4)
U:    addition┌al
A:  →         └not total (.) so its six hundred
              (5.4)
U:    previous value that's six hundr┌ed
A:                                   └ye:s.
              (4.0)
U:    temporary:?
            (1.9)
A:    It has to be temporary: if you ask it (0.6) again if you
      ask for a permanent increase (0.7) it will (.) take much
      lo:nger (would have to authorised by a committee)
```

Such reformulations may involve a change in a unit of talk larger than one word, for example in #2.7.

[UAA.7] #2.7
A: → erm from a uniform distribution between nought and ninety per cent in other wors (.) it's an equal probability. ( ) er for getting the number at any particular value in that range.
(.)
A: [[ge-
U: [[generated a numbers and forgetting it. is that? °(well see)°
A: well. (.) each time you ca:ll it i[t g]enerates
U: [yeh:]
a different random number=
U: =ye:ah:?
A: and if you call it many ti:mes,
U: yeh.
A: → thee (.) numbers will be distributed with equal probability in this ra:nge
U: Y:eh?

In #2.6 the advisor's change, from "additional" to "not total" follows a silence and a minimal response from the recipient. In the case which follow there reformulation appears to occur in the light of intervening talk from the recipient, in other words, the speaker shows sensitivity to the "sequential environment".

[UAA.20] #2.8
A: erm well Helen Brown is our resident (.)
→ kermitologist (1.0)
U: yes
A: → f frog tamer (0.5) em (0.5) and (.) I will refer people with kermit (0.x) [requirements (to her)
U: [um that will she'll know the interface

Here the advisor reruns a joke (kermit, the software being discussed has the same name as a frog puppet in a television programme).

[UAA.9] #2.9
A: do do can it- can it do: one thing (0.4) can it do → this bit first. phi: (1.3) one at a ti::me (.) so will do this bit first (.) and then go for thee second one
(.)
?: °maybe°
(2.8)
U: → can it can it do this integration part first
(.)
A: what with respect to er beta

Here user upgrades his formulation in two ways, substituting a less formal noun: "part" for "bit" and adding an adjective "integration". The following fragments involve reformulations following longer stretches of talk. Incidentally, both

involve the same advisor and the same requirement: that the user join a computer course. In both cases some kind of restatement of earlier material occurs after it appears that getting on a course will be difficult.

```
[UAA.1] #2.10
A:     Ye::s'se there are very few courses left now=
U:     =(hm)
A:     but obv'ously there's only (1.5) I don't know whether
       there's any acshally any courses (.) if you (.) your
       you're a beginner are you= you haven't used (th) computer
       before,
U: →   no well er a little biT
A:     yeh
U:     °in my own country°
A:     ah ye:s
             ( )
A:     well ah'l explain tuh┌ you this
U:                          └This is only one we::( )
A:     you see tha' that's wor:d processing on the BBC which is=
U:     yes
A:     nyeh (be a good one) (0.4) and that one is (.) how to use
       the amda:l computer.
U:     yes=
A:     =but it's for people (0.3) who 'ave used other computers
       beforehand (.) so (0.3)
       ┌it might be┐ a bit-=
U:     └yes    um  ┘
U: →   =I ha:ve used=
       =┌er (                                          ) ┐
A:     └oh well then it- well that's the twenty sev┘enth of April

[UAA.11] #2.11
U:     erh we're second year,
             (0.4)
A:     yeh,=
U:     undergraduate students (0.6) and we >an I've< been advised
        by (.) Mr Roberts of the French department (.) em to do a
   →   (.) one day. course er two day cour:se
A:     y┌eh     ┐
U:      └that's┘ an introduction to the amdahl
             (0.4)
A:     s::- the only one (1.0) this course here,
U:     yeh,=
A:     =people who have u:sed other machines already
U:     oh- (1.5) °um:°
             (3.0)
A:     most of the  ┌courses┐
U:                  └we were┘ under we were under the impression
   →   that there there was some sort of three day course that
       was just before the beginning of ter:m
```

Reformulation in cases such as #2.5 and #2.10 bring about dramatic changes in the recipient. Such cases are doubly interesting since they show the sensitivity to the immediate sequential environment of both parties. There are additional places in my corpus where reformulations bring about changes in the advisor's line.

```
[UAA.15] #2.12
U:     and I've typed in all my text
A:     in script
U: →   yeh in script
A:  →  have you used script before
U:     no
A:     okay
U: →   no it's in wor:d
A:     in wor:d
U:     yeh
A:  →  okay so if it's in word its on a pee see somewhere isn't
       it
```

2.2.4 Advisor's reformulations of user's expressions

A speaker may reformulate what another speaker said in a remedial way. #2.13 is an example of such repair.

```
[UAA.9] #2.13
A:     I I- don't think you should have too much difficulty?
U:     hheh huh huh huh honestly I think so
A:     well
U:  →  I hope so
A:  →  you hope not.
```

In the sessions I find cases of reformulations by the advisor of what the user says which are related to technical matters up for explanation. These may clarify what is going on, or simply perhaps show an understanding of the user's account of their concerns.

```
[UAA.10] #2.14
U:     >the other thing I was going to ask you< is um (0.5) if
       um (.) I'm (at a computer) printing off a particular
       software program (0.7) does it (.) um: disk have to be
       (.) reprinted on a machine using (0.5) exactly the same
       (.) software °program°
             (3.9)
A:     you mean if your using one (.) w[ord pr]oce[ssor a]nd
U:                                    [yes ]    [um:  ]
       then you move t'a diff'rent machine
             (0.4)
U:     yah-
```

[UAA.9] #2.15
U: mm: (.) this w- trivial one and then I wan' to ask something about using some the (.) nag routines wither:::
→ intregral °analysis °
A: [[numerical integra?tion.
(U:)→ [[(yah)]

[UAA.9] #2.16
U: now second thing which I have in mind is I have some double integration (.) on (1.8) I've got a function (.) er: which is (1.2) er (.) phi (.) beta (.) nine. nine so (.) I've g- I've got this I is dependent on (1.2) ph:i: (.) is dependent on (.) phi: and beat (.) phi
(1.6)
A: I is a function of phi and beta phi=
U: =phi and be[ta
A: [okay=
U: now I don't know:w (.) e- exa:ct (.) relationship between (.) I (.) phi and °beta° (0.4)
U: [[but] I got the value of phi: with different values
A: [[( )]
U: of [I:
A: → [so you've got- (0.7) you've got in tabular form in other words y'v got a table of functional value[s]=
U: [yes]
A: =but you haven't got the functional [ru:le]
U: [function]

[UAA.20] #2.17
U: So I'm just going to string a piece of (.) wire
U: [(you know to)
A: [yes
U: → temporary I [I probably want it for y'know half hour
A: [Yes
U: sessioins [that's a:ll
A: → [Yes just now and again (.) it's a typical kermit type of thing (isn't it)
U: yeh

[UAA.8ii] #2.18
A: =well it won't take second (0.4) em- which routine was it you °wanted°?=
U: → =em: it's: (.) dee oh two (.) bee ay eff
A: → bee (.) ay (.) eff (.0.7) I.ll do it for you now (1.8)
→ save you hanging round any °longer° (2.3) rung- runga kutta
U: that's right

[UAA.9] #2.19
A: well yeh I mean that's the way you do it I mean I wanted to see whether there was a (.)
U: → >ready made one<

A: → whether there was a (.) ready ma:de routine you see (0.9)
is funct- function form of integrand known the answer is
n:o:
U: mm hm

Sometimes, to have the right formulation is to have an answer to some question:

[UAA.7] #2.20
U: trying (.) to know (.) I've (.) u:ed? this er. (.)
function
(1.1)
A: yeh (poh)
U: and [that's] (1.1) (the whisk there and I got this
A: [yeh]
brief er signal) er description of er=
A: =ye:uh:=
U: =zero point poi:nt I don't really understand don't
completely understand what is the (.) purpose of this
(values)
(.)
A: ye:uh:. (.) we;ll, (.) er:m: (.) eesh- (.) it's a random
number generator
( )
U: → random generated number in duh- between two lines.=
A: → =tha's right it- th- y- y- you've got- uh- a
[lower a l]ower lower bound an' an upper and
U: [tha's right]
and it generates random numbers (.) in that ra;nge (.)
er but- (.) they're always referred to as pseudo random
numbers (.) okay
U: y:eh?
A: th- the- the difference is qu- was just a matter of
precise terminology °(how) really°=
U: =yeh
A: erm from a uniform distribution between nought and ninety
per cent in other wors (.) it's an equal probability.
( ) er for getting the number at any particular value
in that range.
(.)
A: [[ge-
U: → [[generated a numbers and forgetting it. is that? °(well
see)°
A: → well. (.) each time you ca:ll it i[t g]enerates
U: [yeh:]
a different random number=
U: =ye:ah:?
A: and if you call it many ti:mes,
U: yeh.
A: thee (.) numbers will be distributed with equal
probability in this ra:nge

[UAA.5] #2.21
U: ↑if I am:°a° I have a password in er:m prime
(.)

A: mhm,
U: → can I: (.) go through prime to the amdal
(1.3)
A: no
A: → [you c- you c- y]ou can connect from the prime to the
U: [( )]
A: amdal
U: yes
A: that doesn't give you any additional recources just the same that logging in on an amdal terminal

[UAA.5] #2.22
U: why not make a calculation as example (.) er some times six hundred per week is ten hours
A: yeh
U: I':m not work anything this week why you are not ( ) last week
A: → well it's a moving average: (.) it's not strictly six hundred per week
U: yeh
A: er: if you over run slightly
U: ye[h
A: [if you don't use enough as much th'following week it will average itself out- it's averaged over ten days
U: ah um
A: ((clears throat)) (.) so you're you're running average is still obviously larger than this
U: aha

2.2.4 User adopts advisor's reformulation

There are cases where following reformulation by the advisor a user will adopt that expression.

[UAA.5] #2.24
U: 1→ ( ) after er five it's free::
(1.1)
A: 2→ after six=
U: 3→ =(after six)=

In the following extended example, which includes the whole of a session where I made an enquiry, I adopt the advisor's formulation of the disk that I want. Note that he emphases what this disk is known as, but doesn't say that I was in error. The transcript overs an example of a change in formulation not of a word, but of a whole way of describing the requirements where I switch from talking about my needs to talking in terms of a group (4→ and 7→). I didn't, however, grasp the idea that there is just one filestore which is shared 5→ and 6→).

[UAA.2] #2.25
U: er:: (0.8) person I know (.) in linguistics was able to
1→ send me er, (0.9) sort of dee disk (0.7) um which I'm able to: (0.7) well which is attached t'when I log o:n, which contains a load of files that he's prepared
(0.4)
A: Yeh er:,
U: um (0.4) can (0.5) I set up a whole multitude of disks like that.
(1.2)
A: 2→ w(h)ell: (0.7) that (.) disk (.) there is a permanent disk that er (2.3) ss which belongs to- (.) you you've got there cos you've got a link into (.) a permanent disk.
U: Yeh=
A: =belonging to somebody else
U: Yes
A: Um (1.8) you can't (.) well you can s- have as many disks as you like,
U: Yeh.
A: but um if you want to have them automatically. (0.7) then the owner, (0.6) then the (0.9) owner has (0.9) >°(we:ll I can't- I don't want to say any more than I've got to)<° but- (.) you've got to your directory= you've got to have to get th' directory entry of the machine updated.
(0.5)
U: 3→ well I mean can I set up my own permanent disks.
(0.6)
A: No you ca- well (.) not permanent no.
U: [[(uh:-)
A: [[The whole reason why we have the filestore. (0.6) is (.) >that when you set up a permanent disk you're allocate piece- er-< area of disk
(.)
U: y:eh.
A: which only you can use
U: ye:s
A: 'f y'don't have any files on it.
(.)
U: yes=
A: =it is still in use.=
U: =right yeh (.) this is-
A: and therefore (1.0) uh (1.0) (y'know) it can be wasted so the whole idea of >the whole idea of the filestore< was th'you Get at the disk space when you need it
U: yeah
A: and then files not (in use) are kept in the (.) (format) where they're compressed down and then can be (withdrawn) off line,
U: yeah
A: now (.) if you have a particular need (1.2) for (.) permanent disk space (1.0) you can get some-.
U: ye[s
A: [you can apply for it (0.5) and get it
(0.3)

U: yes
A: and then- that will be (0.7) er put in your dir<u>e</u>ct'ry entry so every time you log on (0.6) you can get it
U: yeah
A: er
U: 4→ well m- maybe my s<u>u</u>pervisor could ask th[e
A: [<u>y</u>eh=
U: =th'reason is that he's he's obtained a load of um (1.1) tr<u>a</u>nscr<u>i</u>pts of >conversations with< young ch<u>i:</u>ldren=
A: =oh I see (ye:ah,)
U: um (.) it's a (.) research group in America and th'-and er >I mean< obviously (1.1) i- it'd be useful to er to er ke<u>e:</u>p them (.) the (best) but yeh obviously (they wont be all the time) (.)
[(available)=
A: [well you c-
A: =I mean you can c<u>o</u>py them across (one at a time) and put
5→ them in the filestore.
U: yeh
A: and then just get the one's you want (and then sort it out?) (.) just get the ones you require at any particular time
(1.9)
U: 6→ wuh you mean have them in your <u>own</u> filestore
A: yeh
U: (half an hour)=
A: =yeh you (only)
you know when you- (8.8) e:m: (1.0) when you go into files- (1.2) to the filestore. ( )
where I say (.) you can put f<u>i</u>les into the pool there.
U: yeh
A: and then wh'never you n<u>ee:</u>d one do a g<u>e</u>t on it
U: yes=
A: =n'then when you finished with it erase it (.) and then it's used so that you can use use ordinary
[work disk in that way
U: [yes
(0.7)
U: yeh well >that's how I mean< o- obviously that's how I usually work but I was just=
A: =yeh but if you w<u>ah</u>nt to have a permanent (0.9) I mean y'super- yes. supervisor or you can put in (.) for a permanent d[isk. (.) and then it will be
U: [yeh
set up (0.9) and=
U: =yes
A: it it will then appear in your (.) in your machine as some
U: ye[h
A: [address say one nine (.) three or something=
U: =yeh
A: um (.) now if its one nine two: (.) it automatically gets linked as d<u>i</u>sk d<u>ee</u> when you log on=
U: =yeh

U: 7→ yeh (.) well I'll check and see how many people wou-
people would be (into the information) >it's just that<=
A: =yeh
U: [[(the ones)
A: [[I think that's that's the critical thing.
U: y[eh
A: [if a whole grou:p of people yes are going to
sha[re]the same information then it makes sense
U: [yeh]
U: ye[h
A: [to have it on a permanent disk
A: but important you can always any time (.) link into any disk you li:ke,
U: yeh.
A: any permanent disk (1.4) em by using a t'link command
U: y:es
A: you spcify: the user name (1.0) er- (0.6) what number that disk is known to 'em
U: [[yeh
A: [[that user ( )
then you can access it afterwards
U: does that mean that you can't sort of er:m: (0.8)
→ protect, (1.2) permanent disks.
A: → well (.) permanent disks. have got pa:sswords on them.=
U: the- they have that's fine=
A: yes so you have so you have to specify a pa- (0.9) if it's going to be a public one,
U: ye[h
A: [you can omit the password.
U: yeah.=
A: see normally. (0.9) er when you er when you do a li:nk, (0.7) it will specify: (0,8) y'know ask (it as) a pa:ssword (.) and there are two passwords. >one for reading one for writing< there are problems with see em ess allowing the people to write to the disk so we that normally give out that one but but the rea:d one. (.) you know it will be (.) will come up
U: but yeh
A: ( )

2.3 Accounts

Students of accounts have generally seen them as explanation for unusual goings on, especially untoward behaviour. For example, Cody and McLaughlin (1985) state that:

> Accounting sequences . . . are manifestations of a systematic provision in the organization of social interaction for the management of *failure events*. Such sequences revolve around the proposition that one of the parties to a relationship has committed an *offence* . . . or has neglected

some obligation, and that a *remedy* should be proposed by the offending party for evaluation by him/her who has offended

An example of this in my data is where a user agrees that he has a file that calls a routine, but then it turns out that he hasn't.

```
[UAA.22] #3.1
A:    do you have an exec to run the program
                (0.6)
U: →  °yeh°
                (8.4)
A:    can't find it (0.9) is it not on the disk
                (20.1)

A:    that's the only (.) exec file you've go:t, (0.4) so you
      don't run it with an exec
                (.)
U:    no
                (1.2)
A:    I thought you said you did
                (.)
U:    oh yeh er s- sorry
      (0.4)
A:    okay (.) so you you run it by doing (0.6) a (.) fort set
      up (.) a fort vee ess essetra
      (0.3)
U:    yeh
      (.)
A:    okay (0.3) would you like to go through that sequence (.)
      and I'll I'll watch
                (2.4)
U: →  I'm er (.) I'm not very (.) familiar with execs
      (0.8)
A:    with
      (.)
U:    execs
      (0.8)
A:    okay well it doesn't matter (.) um (0.6) you know y you
      run through the the can we read the whole of the program
      and em then you load it and try to run it with some data
      I'd like to watch ( ) what happens
```

I want to make a very basic point. Note that at the end of this fragment the advisor provides an account for getting the user to run the program "I'd like to watch ( ) what happens". Such accounts, though hardly for untoward things pervade the talk in the advisory sessions. The following fragments are an assortment of different cases where some kind of account is offered for something.

Inevitably accounts are produced for technical reasons such as the operation of the computer services:

```
[UAA.5] #3.3
A:    and that will be (.) granted (.)
U:    °(aut┌omatically)°┐
A:         └automatically┘
            (0.6)
A: →  but if you ask for more than tha- (0.9) then there will
      be a long delay: (0.8) ┌ cos ┐ it'll have to go through
U:                           └°(hm)┘°
A:    a committee
```

The following cases show how speakers draw on accounts in the course of the exchange. The third one arrowed is interesting since it is offered to explain 1, conduct within the interaction itself and 2, action which has not yet occurred.

```
[UAA.1] #3.2
U:    yeh
A: →  uh 'ts a bit late in the year now
U:    (mm)
A:    most uv th' courses av: f:inished bu we've still got a f:ew

A:    ... your you're a beginner are you= you haven't used (th)
      computer before,
U:    no well er a little biT
A:    yeh
U: →  °in my own country°
A:    ah ye:s
           ( )
A: →  well ah'l explain tuh you this
U:                          └This is only one we::( )
A:    you see tha' that's wor:d processing on the BBC which is=
1

[UAA.8i] #3.4
A: →  right (.) I'll do it in a second we'll just em (2.0) I'll
      just we'll do it now: (.) we'll log in one r- one of these
      terminals over here and do it while this chaps
   → putting in the data.

[UAA.8ii] #3.5
A:    y:::eh (.) um well got a de- copy (0.4) copy i'your
      department.
U: →  no. are (.) been up to: thee: um (0.7) one in th'
      (0.4) mechanical engineering and it's missing out off
      ┌(              )┐
      └((chair creeks))┘
A:    it's what
U: →  missing out of that er set of ┌data f┐iles
A:                                  └ruhah ┘
```

[UAA.8] #3.6
A: what- (0.7) there's something f:unny going on 'ere I I I
I don't know whad id is so (.) em-
(2.1)
A: → do you want to I think I we'll have to look at this some
other ti:[me I- I thi]nk it's to li- i- it'll take a
U: [okay]
A: time to look a[t here] (0.7) you- (2.0) what you could
U: [yeh]

[UAA.11] #3.7
A: → by this time most people have decided whether they want
to use it or they dont't so
(.)
U: y[en]
A: [so] lot's of courses in october november (.) right
through to february but-
[usually there isn't much demand now]
U: → [that's no good to us cos] we'll be over in
France next year:

[UAA.18] #3.8
A: → =okay well (0.4) you log on an we'll (then) see what the
problem is

[UAA.18] #3.9
U: I need to- I've just been getting a message on the
computer screen (0.6) that (0.4) the disk is full. (1.0)
→ erm: (.) an'I don't really know what to do about it so
(0.4)

2.4 Some considerations on getting the session going

I want to close with a brief presentation of two phenomena which may be distinctive of the kind of conversations that take place in the advisory.

2.4.1 Users' presentation of the problem as their problem

Overwhelmingly users formulate their problem as their own problem or their situation.

[UAA.1] #4.1
U: as'd my'er: (.) asked by my er (.) supervisor to: take
some computer course.
A: ah yes s'you[r]
U: [e] given me some information.
A: yes well these are the courses we've go:(t),

[UAA.2] #4.2
U: As it happens I've got a (0.3) problem myself tha' I
could ask you about it,

```
                (0.9)
U:      e[r
A:       [right
                (0.1)
U:      er:: (0.8) person I know (.) in linguistics was able to
        send me er, (0.9) sort of dee disk (0.7) um which I'm
        able to: (0.7)  well which is attached t'when I log o:n,
        which contains a load of files that he's prepared
                (0.4)
A:      Yeh er:,
```

```
[UAAX.8i] #4.3
U:      Excuse me (.) em:: (1.0) I'm (.) interested in thee;
        >introductory< (.) course (.) on the amdahl on the (.)
        [twenty seventy of ap] ril and [I
A:      [y: : : : : : : : :  ]         [oh . do you want me to
register you.
U:      please (.) I['ve got a] user name
A:                  [ okay  ]
```

```
[UAA8.ii] #4.4
U:      'scuse me (0.40 is there any chance I can have a
        ↑photocopy of one of these er (1.2) [(graph) can I do it
A:                                          [er-
A:      y:::eh (.) um well got a de- copy (0.4) copy i'your
        department.
U:      no. are (.) been up to: thee: um (0.7) one in th' (0.4)
        mechanical engineering and it's missing out off
        [(                  )]
        [((chair creeks))   ]
A:      it's what
U:      missing out of that er set of [data f] iles
A:                                    [ruhah ]
```

Some users even classify themselves as a preliminary to explaining their business. This could be because they know, as I mentioned above, that the user access area in which the advisory is places is not intended for use by undergraduates. This is relevant in the next example:

```
[UAA.10] #4.5
U:      right hello? (.) um:?
                (3.4)
A:      do you want it ((the chair))
                (1.3)
U:      (  ) s'long as it's no hassle
                (.)
U: →    um I'm an undergraduate=
```

A: =yeah=
U: um (.) at the university an I've got a dissertation (0.5) um which I (0.6) like to:: °um° print up ona word processor
A: y:eah

[UAA.11] #4.6
U: → erh we're second year,
(0.4)
A: yeh,=
U: → =undergraduate students (0.6) and we >an I've< been advised by (.) Mr Roberts of the French department (.) em to do a (.) one day. course er two day cour:se
A: y[eh
U: [that's] an introduction to the amdahl

Such formulations are not inevitable, however, the following two fragments are exceptions. Interestingly, the user's opening question, which is not couched in first person terms, is not given a serious reply. Incidentally, the user has already shared a joke as the previous session came to an end. The user soon produces a first person story as he narrows down what the problem is.

[UAA.20] #4.7
A: Y[eh
U: → [RIGHT(.) what do you know about kermit(h) (.) and opu[s (pee see) (.) pee see fives:
A: [um uh::m
A: Well I er I kn-(.) what I (.) do know I copyright (.) er
U: ((lau[gh)) .
A: [((laugh)) ((1.2) copyright (1.2) what)
(2.5)
A: [[er
U: [[all I want to (do:) is h:ow I can: (1.5) use
(know)
kermit on the (1.3) pee see five dw'I need any additional: chips or:: anything or [(0.7) will[the
[no [no
U: software just=
A: =It'd just be the software:

In the next fragment the speaker adopts, and sticks to, a non-first person account.

[UAA.21] #4.8
U: → it suddenly decided not to work anymore.
(0.9)
A: it suddenly decided not to work anymore.=
U: =yes
A: [[oh
U: [[it was working fine (an em)
A: yeh okay (0.7) you you've obviously made some changes to it

```
                (0.6)
U:      er no: (0.6) just suddenly decided not to work anymore
                (.)
A:      oh kay, when did you last use the program
                (0.6)
U:      er:: (1.7) yesterday
                (0.2)
A:      yesterday and it's stopped working since then
                (0.5)
U:      yep
```

2.4.2 Aspects of the advisors' response

Often the turn during which the advisor responds to the users formulation of the problem contains hesitations and restarts, which for convenience I will gloss as "dysfluencies".

```
[UAA.7] #4.9
A:      (                    )
                (5.4)
A:      Is your's a very quick question,
                (0.5)
U:      Y:es.
                (.)
A:      Yes. go on. then,
                (2.4)
U:      trying (.) to know (.) I've (.) u:ed? this er. (.)
        function
                (1.1)
A:      yeh (poh)
U:      and [that's] (1.1) (the whisk there and I got this
A:          [yeh   ]
        brief er signal) er description of er=
A:      =ye:uh:=
U:      =zero point poi:nt I don't really understand don't
        completely understand what is the (.) purpose of this
        (values)
                (.)
A: →    ye:uh:. (.) we;ll, (.) er:m: (.) eesh- (.) it's a random
        number generator
                ( )
U:      random generated number in duh- between two lines.=
```

Yet at other times the advisor comes straight out with a fluent response.

```
[UAA.1] #4.10
U:      as'd my'er: (.)  asked by my er (.) supervisor to: take
        some computer course.
A:      ah yes s'you[r
U:                  [e given me some information.
A: →    yes well these are the courses we've go:(t),
```

U: yeh
A: uh 'ts a bit late in the year now
U: (mm)
A: most uv th' courses av: f:inished bu we've still got a f:ew

What could be going on here? A number of different things might be possible. Certainly the advisor's task in each of these two examples is very different. In the first one the formulation of the user's requirement is harder to grasp than in the second one and a response harder to formulate since there is uncertainty about what kind of knowledge the user will have. But though this might account for why giving a response is difficult here it does not explain why the advisor is not fluent. I want to examine some other cases to try and get at why this might be. Firstly this dysfluency is not idiosyncratic, for the same advisor as in #4.10 is speaking here:

[UAA.2] #4.11

U: As it happens I've got a (0.3) problem myself tha' I could ask you about it,
(0.9)
U: e[r
A: [right
(0.1)
U: er:: (0.8) person I know (.) in linguistics was able to send me er, (0.9) sort of dee disk (0.7) um which I'm able to: (0.7) well which is attached t'when I log o:n, which contains a load of files that he's prepared
(0.4)
A: Yeh er:,
U: um (0.4) can (0.5) I set up a whole multitude of disks like that.
(1.2)
A: → w(h)ell: (0.7) that (.) disk (.) there is a permanent disk that er (2.3) ss which belongs to- (.) you you've got there cos you've got a link into (.) a permanent disk.

How tightly specified the question is seems to be relevant:

[UAA.20] #4.12
U: See how trusting you a:re with a piece of stri:ng?
(0.8)
A: well:
(0.6)
U: ((laughs))
A: I(.)it(s) it is not the value of it (.) it's the availabilty of it that's important
U: Caught me

(1.4)
A: Yeh okay thanks u:m (0.9) le let me just put a ring round it (0.5) er
(11.5)
A: thanks okay (0.4) y'n(.) somebody will send you a mail note about it 'ts the best way(h)
UO: yeh thanks very m[uch?
A: [okay bye
(1.4)
A: Y[eh
U: [RIGHT(.) what do you know about kermiT(h) (.) and opu[s (pee see) (.) pee see fives:
A: [um uh::m
A: → Well I er I kn-(.) what I (.) do know I copyright (.) er
U: ((lau[gh))
A: [((laugh)) ((1.2) copyright (1.2) what)
(2.5)
A: [[er
U: [[all I want to (do:) is h:ow I can: (1.5) use (know) kermit on the (1.3) pee see five dw'I need any additional: chips or:: anything or [(0.7) will [the
[no [no
U: software just=
A: → =It'd just be the software:

Whilst other enquiries about courses get prompt responses

[UAAX.8i] #4.13

U: Excuse me (.) em:: (1.0) I'm (.) interested in thee; >introductory< (.) course (.) on the amdahl on the (.) [twenty seventy of ap]ril and [I
A: → [y: : : : : : : : :] [oh . do you want me to register you.
U: please (.) I['ve got a] user name
A: [okay]

[UAA.11] #4.14
U: erh we're second year,
(0.4)
A: yeh,=
U: undergraduate students (0.6) and we >an I've< been advised by (.) Mr Roberts of the French department (.) em to do a (.) one day. course er two day cour:se
A: y[eh]
U: [that's] an introduction to the amdahl
(0.4)
A: y:es
(0.6)
U: an: we were told to come and see you to (.) [find out when
A: [book up]
(.)
A: → yes well the cou[rses are] (0.7) there aren't very many

U: └times °av┘ailable
courses this year, (2.0) am: (10.6) there are no (.)
completely basic courses. (1.0) for beginners left this
year

The following fragments suggest that there are constraints on how long a silence can be left before a response of some kind has to be provided or on how much explaining can be expected form the user:

[UAA.18] #4.15
U: I need to- I've just been getting a message on the
computer screen (0.6) that (0.4) the disk is full.
→ (1.0)
erm: (.) an'I don't really know what to do about it so
(0.4)
A: ri'[ght] o┌ kay
U: └I think that you have to save it or something
like that

[UAA.4] #4.16
U: No I wouldn't waste er (.) either of our time with
>a contrived< one. (0.4) em (1.2) and a- (0.4) well
I think I know the answer. (0.5) and that's that
(0.3) I think the answers negative. (0.8) em I'm trying
to (1.3) send er:m (0.8) BBC files in view,
(0.3) >well in fact you only had a phone call about this
a moment ago<=
A: =°mm hm°=
U: =em (0.8) view (0.5) output files. (.) to thee
(0.8) amdahl (0.2) em (1.1) now (.) >as far as I know
there
are two ways of doing this< one is using a kermit (0.8)
where you have to em (0.5) spool them into (0.7) file
stripper (1.0) control
characters (an then se┌nd it through) kermit
(A: └right)
(0.6)
U: and then the alternative is to use thee (0.7) em
(0.6) the host (.) facility using the write
┌on that (0.6)
A: └yeh
U: now (0.7) the the problem is that the files I've got are
very bi:g (0.4) they're too big to (0.2) all be read into
(0.4) the memory of a BBC micro. (0.5) which means that
neither of those methods wo:rks (.) >the kermit thing
doesn't work straight off because< (0.5) to: spool it (.)
with (.) the pee (.) spool (.) are (.) that removes
the (.) >control characters< you need to have it
all all in memory °I think°
A: °mm hm°=
U: =and then also to use the host thing you have to
(0.8) have the file (.) in the memory.
(0.5)

A: → no the well. (1.4) there is- (0.7) there is a mechanism whereby you can (2.1) you can stage it off a disk (0.6) with (.) vie:w (.) o:n >host<
(1.3)

[UAA.10] #4.17
U: right hello? (.) um:?
(3.4)
A: do you want it ((the chair))
(1.3)
U: ( ) s'long as it's no hassle
(.)
U: um I'm an undergraduate=
A: =yeah=
U: um (.) at the university an I've got a dissertation (0.5) um which I (0.6) like to:: °um° print up ona word processor
A: y:eah
U: (in advance of) handing it in=
A: =yeh=
U: → I was (.) wondering if thee (.) university has facilities to do things- to do tha'
A: → well. (1.6) there are, (1.0) >see the've go'-< (0.7) these (0.6) micros up here are (restricted use) (0.8) particularly for research purposes (0.6) theory j' research [only] (0.6) is that the problem (0.5) er:m
U: [(yeah)]
( ) some uv thuh departments have goT (0.8) er which department are you from
U: I'm from law: akshually (.) and they don't have (them)

[UAA.2] #4.18
U: er:: (0.8) person I know (.) in linguistics was able to send me er, (0.9) sort of dee disk (0.7) um which I'm able to: (0.7) well which is attached t'when I log o:n, which contains a load of files that he's prepared
(0.4)
A: Yeh er:,
U: → um (0.4) can (0.5) I set up a whole multitude of disks like that.
→ (1.2)
A: w(h)ell: (0.7) that (.) disk (.) there is a permanent disk that er (2.3) ss which belongs to- (.) you you've got there cos you've got a link into (.) a permanent disk.

[UAA.7] #4.19
U: =zero point poi:nt I don't really understand don't completely understand what is the (.) purpose of this (values)
(.)
A: → ye:uh:. (.) we;ll, (.) er:m: (.) eesh- (.) it's a random number generator

( )
U: random generated number in duh- between two lines.=

These examples suggest a relationship between offering a personal account and the advisor's dysfluency. In these examples although the users have sketched quite detailed concerns they feel it necessary to offer autobiographical supplementaries "I don't really now what to do about it", "I was wondering if the university had facilities to do that", "can I set up a whole multitude of disks like that", "I don't really understand the purpose of these values". Such devices appear to be effective in placing the responsibility for further talk with the advisor.

3.0 Conclusion

Drawing on detailed transcripts and on some findings in conversation analysis I have 1, tried to understand a cooperative conversational phenomenon and to apply an analysis of it to some features found in the advisory sessions. 2, shown that at the level of lexical selection speakers manage reformulations on a routine basis and appear to orient to the sequential environment where they speak. In this sense the choice of tėrm can be understood as being under a constraint which is communicative in nature. A separate point is that 3, within the explanatory business of advice talk explanations routinely occur in the form of accounts. A whole range of things become the business of accounting. Finally 4, I gave examples of two things which in general appear to characterise some of the constraints operating at the start of the sessions: the advisor's dysfluency in the initial response to the user's problem and the user's adoption of a personal style of narrative.

Although I drew attention to the role of accounts in the advisory sessions my analysis has departed from the path marked out by the studies in chapters two and three in two ways. Firstly, in those chapters the focus of analysis was on discrete explanatory utterances, whereas here, although I have taken data from episodes where explaining is going on, I have not tried to identify and analyse isolated expressions which encapsulated an explanation. Secondly, in connection with the style of work, those earlier studies involved pushing certain conceptual claims by appealing to data derived from conversation. In this chapter the force of inquiry has been more tentative and exploratory. I want to emphasise that my analyses here are not representative of the detail and subtlety which an experienced conversation analyst could bring to bear. As I pointed out at the start of this chapter, I have been concerned with illustrating some of the ways in which the conversational environment appears to constrain what participants in conversation do.

A topic for further exploration would be the analysis of explanatory utterances themselves, elucidating the role that they have in explanation giving and how the speech situation constrains their function and formulation. In the remaining chapters, however, I want to examine, in different ways, the sense in which communication and what I have called communicative constraints are social phenomena. In the next chapter I want to pursue the idea that the participants have knowledge of a way of behaving which is normative in nature and is sensitive to the concurrent details of the interaction by taking up, very briefly, a feature the nonvocal activities in these advice sessions.

CHAPTER 6: SOME OBSERVATIONS ON THE ORGANISATION OF BODY MOVEMENT IN COMMUNICATION

I want, briefly, to comment on the appropriatenesss of the idea that participants' behaviour is normative (rather than rule governed in a quasi-grammatical sense). I discuss this in relation to a set of findings on change in body-posture during telephone interuptions to the advisory. During such calls the advisor and user typically move apart, and then move together again once the call is over.

1.0 Introduction

It is obvious, reflecting on our ability to walk along a busy street without bumping into people, that we are sensitive to other people's spatial positioning. It would be surprising if such a capacity did not have a role in the conduct of communication. In addition to taking enquiries from users coming to it, the advisory also receives telephone enquiries. When a phone call first occurred during a session that I was recording I initially viewed this as a problem for my data collection. But of course it is also a problem for the participants which requires management. Although the phone call enquiries themselves are not researchable (only one party can be heard of course) the impact of the call on the session in which it occurs turns out to be highly interesting. Watching the video tapes shows that the phone call is a site of bodily repositioning of the advisor and user. Acknowledging the complexity of face to face communication much of the early work in conversation analysis was carried out on telephone call conversations because in an audiotape of a phone call the analyst has all the material that was available to each participant. (See for example Schegloff's work on "Sequencing in conversational openings" (Schegloff, 1968)). My situation ironically complements such data. Again in acknowledgement of its complexity I will only attempt a very coarse analysis of a gross phenomenom. On some occasions the advisor phones out for information. The data showed nine cases of telephone calls altogether, seven involving calls out, two calls into the advisory.

1.1 Relationship to previous work on body movement in communicative situations

I have already mentioned work on body movement in earlier chapters. For example, Marsh et al (1982), used video recordings to establish patterns in the distribution of football fans in the stands. Duncan and Fiske (1977) included body movement categories in their "external variable study". Birdwhistell established a field called "kinesics", the study of body movement in communication. This approach involved attempting a comprehensive description of body movement in interactions (Birdwhistell, 1970).

Conversation analytic work on nonvocal phenomena (Heath, 1984, 1986; Goodwin, 1981, 1984; Schegloff, 1984) differs from most social psychological work in that quantitative observation is avoided in favour of trying to examine the structure of body movement which is construed as interactively organised. Here too the idea that phenomena of interest are produced and attended to by the participants is used as a guiding principle.

Heath (1986) presents an analysis of medical interactions. In his chapter on "Display of recipiency and beginnings of consultation" he includes the following fragment (p. 38):

```
(1)
Dr:    and they help (.) at the time
             (0.5)
P:     yeh (o.kay)
             (1.5)
P: →   but I haven:'t (1.2) he gave me seven to take it
       dow:┌:(n)
Dr:        └mm huh
             (.)
P:     taking them like that::t
```

Heath's analysis focuses on the pause in the patient's talk (in the arrowed section). Paying attention to the non-vocal environment in which this utterance occurs shows that it has an interactive dimension to it. At the start of this turn the doctor is reading the medical records, but after one second starts to move his gaze to the patient. When his gaze reached her face she continues speaking. The patient is withholding talk until the doctor's gaze returns.

```
(2)
P:      but I haven:'t ----------+--he gave me seven to take
        _ _ _ _ _ _ _ _ _ _ _ _ _'——————————————''
        ↑
        reads records
```

Here I shall not attempt an interaction analysis of the non-vocal behaviour. I will only describe the phenomenon that I want to at a very coarse level, using a few illustrations.

2.0 Analysis [1]

2.1 Basic finding

The basic finding appeared to be that during any phone call the advisor and user would separate for the duration of the call and then move back together when it was over. The movements are highly complex. Repeated viewing reveals that each movement has a number of components, suggesting the possibility of an analysis in terms of how the movements of each participants relate to each other. I will not attempt such an analysis here. Figures 6.2, 6.3, and 6.4 illustrate the basic finding. (These drawings are produced by tracing onto an acetate sheet placed over a television screen. This method is fast, and is far cheaper than photographing the screen or use of an image analyser. Furthermore the drawing can be readily photocopied. Of course the drawings are very simple and are simply intended to sketch the behaviour in question.) The figures 6.1 and 6.2 show the positions taken up before, during and after the phone call. The user in figure 6.3 carried out an extended movement stretched over a few seconds. This figure show the positions of user and advisor before the call, the final position adopted by the user (lowermost) and an intermediate position. Appendix 7 gives brief details of the users in the sessions studied

---

[1]I am grateful to Colin Clark, Dept of Psychology, University of York, for discussing these data with me.

FIGURE 6.1 SKETCHES OF BODY POSITIONS BEFORE, DURING AND AFTER A TELEPHONE CALL INTO THE ADVISORY [UAA.3]

FIGURE 6.2 SKETCHES OF BODY POSITIONS BEFORE, DURING AND AFTER A TELEPHONE CALL OUT OF THE ADVISORY [UAA.6]

FIGURE 6.3 SKETCHES OF BODY POSITIONS BEFORE, AND DURING TELEPHONE CALL INTO THE ADVISORY [UAA.9]

2.2 Development of the analysis

I want to supplement this basic finding by adding brief verbal descriptions of the other cases and then by considering some cases which show variations on the pattern that I reported.

2.2.1 Verbal description of movement

Since the movements concerned are complex and difficult to transcribe all I have attempted to do here is list what I would point out to someone watching the video data itself. I offer these as a brief record of how many calls occurred and whether the call was made out of or into the advisory.

1, UAA.3 in

Illustrated in figure 6.1. The user is invited to carry on trying something on the terminal which he initially does, until he adopts the middle of the illustrated positions. After the call the user and advisory move back to look at the terminal together.

2, UAA.6 out

Illustrated in figure 6.2. The advisor moves considerably, leaning right back in his chair with his legs outstretched. The user adopts the position illustrated and holds it throughout the call.

3, UAA.8 in

Illustrated in figure 6.3. The user makes a dramatic movement made up of a number of stages.

4, UAA.14 out

Here the user remains standing during the start of the session. He is standing when the advisor phones out and remains in that position during the call until the advisor discovers that he has misidentified the person at the other end of the line. At that point the user walks off and returns with some documents which he looks at standing in his initial position.

5, UAA.15 out

In this instance the user is seated opposite the advisor. When the advisor phones out she takes off her coat.

6, UAA.17 out

See discussion of the session below.

7, UAA.19 out

During the call the user sits back in his chair and grooms his beard.

8, UAA.20 out

At the start of the call the advisor and user look at each other. Once the call is underway the user grooms his beard and joggles a box of disks which he has in his right hand.

9, UAA.21 out

User grooms hair as call starts, then shifts in his chair once the call is underway.

10, UAA.24 out

As in UAA.14 the user is standing on the advisor's right, leaning on the desk. At the start of the call the user stands more erect and then walks off. He returns and adopts his original position when the call finished.

2.2.2 Additional movements

The findings reported above require elaboration. Some of the interactions mentioned above involved other movements which are possibly related to the ones that typically occur during phone calls.

1, The position taken up in figure 2, UAA.6 occurs elsewhere independently of phone call. Figure 6.4 shows the positions before and just after the advisor invites another user if his problem is a short one. Again, as the session nears its end the user once again takes up his position, lowermost of figure 6.4. The user also adopts this position when another person is attended to by the advisor. The sketches show that despite the similarity in the posture the users body is oriented differently in these cases.

2, The result of the phone call made by the advisor in UAA.15 is that a document that the user wants to have printed can be done straight away. The advisor explains this and requests that he drinks his coffee before he shows her where she has to go. At this point the user looks away from him and as he drinks she busies herself with some papers.

3, UAA.17 is an exceptional case in so far as the advisor speaks to the user at two points whilst on the phone. At these points the user nods stiffly and leans forward slightly, keeping his back straight. This contrasts with the movement that he makes towards the advisor after the call is over.

FIGURE 6.4 SKETCHES OF BODY POSITIONS BEFORE, AND WHILST THE ADVISOR ADDRESSES ANOTHER USER, PLUS POSITION ADOPTED NEAR THE END OF THE SESSION [UAA.6]

3.0 Interpretation of findings

I think that it is clear that the behaviour observed is interactively organised and locally managed. The fact that such organisation occurs and in such situations is interesting in its own right but I want to consider the implication of such behaviour for rule-governed models of human behaviour. When attempting to explain the structure of conversation some students have been tempted to draw on quasi-grammatical accounts, for example Clarke and Stubbs. I hold that such accounts can't be correct because the kind of grammar that can be said to apply in the case of an utterance is pitched at the wrong level to describe an interaction or string of utterances. It is pitched at the wrong level because in the case of a single utterance the grammar describes an aspect of how elements of that utterance go together to give it its distinctive structure. Now, irrespective of the impact of concurrent speaker - recipient interaction, an utterance is fundamentally something which one party puts together to display to another, even if those parties are the same person. Since a different arrangements of elements could constitute a different utterance: just how the elements are put together is crucial to the speaker's intention, goals, or whatever. Such structure, though it may perhaps relate to the structure of a story, has no counterpart in the structure of an interaction. In an interaction the emergent structure is the collective result of two, or more, parties. Whilst each utterance constitutes an ordered display for another party the interaction as a whole does no such thing. In certain cases people might "look back" on an exchange and analyse it too see just where it went wrong, but speakers are not generally in the business of putting together a show which is surveyed at the end to see what the result of it is. What then motivates or organises each element of the interaction, each turn, move or utterance? The truth is that we really don't know in any detail. Besides what would an account of it involve?

Whatever the answer is, it must start from a practical knowledge of what is going on in the situation in which interactants find themselves and in a knowledge of what is appropriate conduct to some end, be it to comply or resist. That people in interaction have such a commonplace yet sophisticated capacity is something which I think the brute data of the body movement which I have

discussed leads us to. For such data, not being a propositionally differentiated sign or any such thing, is simply people co-interacting in orientation to some practical understanding of what fits in that situation. Such conduct may be described in terms of acting by orientation to a norm.

4.0 Conclusion

The method of analysis that I have applied is of course very simple. The structure of such behaviour could be further described by a transcription method such as the one used in the fragment quoted from Heath above. I have experimented with transcribing some of these episodes in such a way and certainly the detail which emerges when repeated viewing is coupled with some means of representation is astonishing and I have resisted the temptation to take up the challenge of pursuing these themes. The analysis is, I think, sufficient to illustrate a practical capacity which is unsuitable for having a quasi-grammatical account grafted onto it. I think that such behaviour points to a basic interactive capacity which lies prior to and underlies behaviour that a rule-governed account might wish to describe.

CHAPTER 7: COGNITIVE AND INTERACTIVE PERSPECTIVES IN THE DESCRIPTION OF COMMUNICATIVE BEHAVIOUR

In taking as my research topic utterances rather than beliefs and in suggesting that what people say shifts from situation to situation I could be said to have displaced the cognitive phenomena by social ones and to have fragmented the concept of a person. Although some writers do adopt a non-cognitive stance I want to distance myself from such a position. I start from a discussion of some of the uses to which terms like "frame" and "script" have been put. Broadly speaking, when examining accounts of conversation in the literature there is a split between cognitive psychologists and artificial intelligence researchers who adopt cognitive models and sociologists and anthropologists who adopt interactive accounts. Ironically the term "frame" occurs in both literatures, but is used in different ways.

1.0 Schemata and frames

There is a view which holds that there is a reality too complex to be understood (Plato's cave) or is instrinsically meaningless. In either case it is held that what we do when we perceive or when we understand is to fit reality to some conceptual scheme. Kant examined what he took to be a fundamental system of categories through which the "manifold of experience" can be thought. James held that the world of the neo-nate is a "blooming buzzing confusion" which the child had to learn to perceive. This paradigm of an input which requires structuring guides some of the protagonists to be discussed:

A broad distinction separates some of the positions to be considered. It is this: on the one hand 1, the representation of knowledge or the organisation of the mind (here we find most work in the cognitive sciences, Minsky, Schank, most cognitive psychologists) on the other hand 2, the presentation of the self in the world, the organisation of experience (this is the position taken up by most of the social scientists which I will consider, Goffman, Garfinkel). The distinction is a profound one since it separates students who enquire into how the world could be represented from students who would rather ask "what is the social

world like?".

I think that some psychologists occupy a sort of middle ground where there are perspectives which investigate how the world is construed by individuals but which neglect 1) the organisation of the cognitive structures and 2) the coordination of action in the world. Examples here are Kelly's personal construct theory and attribution theory. Both these approaches appear to spring from Lewin's influence on American social psychology. I suggested in chapter one that this is a strain of phenomenology which has bypassed the work of Schutz and Garfinkel. This amounts to idealism in a realist world. Also in the middle ground, though closer to the perspective of "the organisation of experience" than "the organization of knowledge" and more concerned with action than construct and attribution theory, is symbolic interaction, going back to G.H. Mead.

1.2 Schema

In the transcendental psychology of the Critique of Pure Reason Kant uses the term "schemata" but in a rather different way to its use in psychology. His idea of a "category" is more closely related to the idea of a schema as it developed below. For Kant the schemata mediate the application of the categories to experience. For Kant both schemata and the categories (of which there are precisely twelve) are void of empirical content, they are a priori, transcendental structures which are discovered by Kant in his search to explain how given that thought is possible, how it is possible.

F.C. Bartlett saw recall as a creative and a social process. Bartlett is associated with the term "schema", although he gets it from the neurologist Head. In fact Bartlett criticises the term saying that it already has many and vague uses and he clearly has reservations about using it. Head wished to describe our knowledge concerning limb position in the course of his work on afferent nerves. Because of the perpetual novelty of different situations he wanted to use a term to account for the plasticity of a person's know-how. Bartlett follows him in this. He wanted to describe the meaningful errors which occured when a story

was recalled by a subject. Bartlett wanted to stress that memory does not consist of a set of discrete units.

The arrival of the term "schema" from physiology is ironic since the term is used by Piaget in his genetic account of knowledge - the genesis being in the infant's coordination of bodily movement with environmental happenings: the development of sensorimotor schema. The term schema is still common in developmental psychology. A further irony is that the French word used by Piaget could be translated as scheme and that he has subsequently used the term "schema" for a slightly different purpose. (Ginsburg & Opper, 1969, p.20). Neither Piaget nor Bartlett set out to offer a detailed account of how schema achieve what they do.

1.3 Frames

The idea of a "frame" as an organising entity occurs in two different senses.

1.3.1 Organisation as the representation of knowledge: cognitive science

The classic text here is Minsky (1975). This paper sets out an outline of a theory of frames

> When one encounters a new situation or makes a substantial change in ones view of the present problem) one selects from memory a substantial structure called a frame.

He describes this as a remembered framework adapted to fit reality and that it is a "data structure for representing stereotypical situations" He claims that his frames follow the Bartlett "schema" and Kuhn's "paradigm". I don't see that this is so 1, He restricts it to novel situations, not true for Bartlett or Kuhn 2, the ideas operate in quite different ways. Anyway, he thus views the frame idea as unoriginal but suggests that his idea of a frame system might be original.

> Collections of frames are linked together to form frame systems. The effects of important actions are mirrored by transformations between frames of a system. These are used to make certain kinds of calculations economical, to present changes of emphasis and attention...

Abelson (a psychologist rather than a computer theoretician) invokes what he calls a script. Abelson (1978 p.33):

> The theory rests on the idea of a cognitive script. By script I mean a coherent sequences of events expected by the individual, involving him either as a participant of an observer. Scripts are learned throughout the individual's lifetime both by participation in event sequences and by observation of event sequences...Because individuals have different histories, they may have different scripts, although some scripts may be so overlearned that they may be universal. I hypothesise different scripts, varying in length of time frame, the number of active characters, and whether or not the separate events in the scripts are concrete or generic.

He states that scripts are made up of chains of "vignettes" which "in short, represent the raw constituents of remembered episodes in the individual's experience". His central claim is that scripts offer a better account of the representation of knowledge than propositional networks do.

The script idea is developed in Schank and Abelson 1977. To it they add the idea of a plan, "A plan is intended to be a repository for general information about how actors achieve goals". They do this by setting out the choices available to a person in whatever situation is being considered. Invoking a plan enables a reader of a narrative to connect disparate parts of a story together.

The project here is to develop a finite list of "concepts" which if represented in a machine would enable the machine to store information arriving in the form of a narrative and sortable by those concepts. The book raises some interesting, but rather obvious questions, about what is involved in understanding a narrative about an everyday event. The emphasis is on a simple formal model of such phenomena rather than say, a conceptual examination of human action, an empirical study of what sort of things people act on and interpret their world through, or an examination of the problems involved in interpreting human action. These topics are taken up by the researchers discusses below.

1.3.2 The organisation of experience and action: the behavioural and social sciences

Work in these areas is characterised by two themes: 1, the view that communica-

tion involves messages and actions of different types which must be identified as such; 2, this is a problem faced by participants and students of social life.

In some social scientific writing a radically different conception of "frame" to that developed by Minsky, and one that predates his, can be found. Minsky thinks of frames in the sense of scaffolding, here we move to frame in the sense of a picture-frame, something demarcates or "brackets".

In "A theory of play and fantasy" Bateson (1955) notes that the communication of humans and some animals involves signals about messages or behaviours. Thus otters can deliver friendly nips and reconciliation ceremonies contain ritual blows. The paper moves from ethological and anthropological examples to psychopathology and psychotherapy via logical theory and a consideration of how these metacommunicative phenomena operate at a different level to the behaviour to which they refer. Bateson holds that these signals delimit or frame behaviour. He does not develop a theory of frames. He is concerned with the status of framing signals especially the case of paradoxical messages. His attention is on how a disruption of the framing process could be involved in the aetiology of psychopathology. This idea is, of course, developed in the Palo Alto theory of schizophrenia.

The theme of signals and messages dove-tails with the idea of construing families as systems (of communicating members). The term context is invoked here to make the point that an individual's behaviour takes on a different meaning when their family situation is grasped.

Goffman is well known for his "dramaturgical" account of social life (Goffman, 1955, 1969). In Frame Analysis he (1974) draws on Bateson's work though a structural element is introduced. Goffman attributes an organising role to frames and invokes them to account for the different ways of seeing a situation. Incidentally he uses "schemata" and "schemata of interpretation" synonymously with "frame".

> And of course much use will be made of Bateson's use of the term "frame". I assume that definitions of a situation are built up

> in accordance with principles of organization which govern events - at least social ones - and our subjective involvement in them; frame is the word I use to refer to such of these basic elements as I am able to identify. That is my definition of the term frame. My phrase "frame analysis" is a slogan to refer to the examination in these terms of the organization of experience. (p. 10-11)

The book 1, sets out a position on what social reality is, this involves a brief critical account of Schutz and James; 2, offers a set of conceptual tools; 3, offers empirical data in the form of illustrative accounts culled, in a confessedly serendipitous manner, from magazines and newspapers. The disagreement with James is that he holds that what is real is what shows itself to be real (the classic pragmatist position developed by Dewey as "warranted assertability"). Thus we have a plethora of fictive domains or subworlds. Schutz is viewed as remaining within this position but adding to it an account of how subjective worlds are created and maintained. To this phenomenology of "experience structures" Garfinkel, a point that came up briefly in chapter one, is viewed as adding the idea that we can discover rules which constitute multiple realities.

Goffman presents what he calls primary frameworks which he describes as follows:

> When the individual in our Western society recognizes a particular event, he tends, whatever else he does, to imply in this response (and in effect to employ) one or more frameworks or schemata of interpretation of a kind that can be called primary. I say primary because application of such a framework or perspective is seen by those who apply it as not depending on or harking back to some prior or "original" interpretation; indeed a primary framework is one that is seen as rendering what would otherwise be a meaningless aspect of the scene into something that is meaningful (p.21)

Such frameworks are held to vary in their degree of organisation. Some being presented as systems of entities, postulates and rules; other having "no apparent articulated shape, providing only a lore of understanding, an approach, a perspective." In Frame Analysis

Goffman invokes the ideas of keys and keyings to give an account for the switching of frames (p.45). This topic is subsequently developed in a paper called "Footings":

> 1. Participants' alignment, or set, or stance, or posture or projected self is somehow at issue. 2. The projection can be held across a strip of behaviour that is less long than a grammatical sentence,

or longer, so sentence grammar won't help us very much, although it seems clear that a cognitive unit of some kind is involved, minimally, perhaps, a "phonemic clause". Prosodic not syntactic segments are implied. 3, A continuum must be considered, from gross changes in stance to the most subtle shifts in tone that can be perceived. 4, For most speakers, code switching is usually involved, and if not this then at least the sound markers that linguists study: pitch, volume, rhythm, stress tonal quality. 5, The bracketting of a higher level phase or episode of interaction is commonly involved, the new footing having a liminal role, serving as a buffer between two more substantially sustained episodes. (1981).

2.0 Cognitive versus interactional accounts

Apart from a conflicting use of the term "frame" these two approaches, the artificial intelligence and the social scientific one, simply seem independent interests but there is a latent conflict between them, depending on how they are developed. If in some interaction scene the participants are construed as information processing units, then the problem of rescuing this from being a highly individualistic account would rest on an ability to elaborate and program an immense amount of interactive know-how which would need to be investigated precisely by the study of interaction itself.

Potter and Wetherell make strong claims about the nature of discourse analysis:

> Our focus is exclusively on discourse itself: on how it is constructed, its functions, and the consequences that arise from different discursive organization. In this sense, discourse analysis is a radically non-cognitive form of social psychology. (p. 178)

The position that they wish to adopt is not that mental phenomena are of no interest -indeed they demonstrate considerable interest in mental discourse. This is the familiar position promulgated by Ryle and Wittgenstein. The sense in which their work is non-cognitive is then presumably that the research data and theorising remain at the level of traces of discourse. As I indicated earlier, despite talking of a tripartite project of discourse analysis involving speech act theory, conversation analysis and semiology, and despite talk of an interest in how discourse is put together the analyses offerred do not get beyond the

identification of variability in accounts. We are dealing with a textual model of discourse analysis, taking as its principal tool an issue which has been familiar since Aristotle - being on the look out for contradictions in an argument or position. My disappointment with Potter and Wetherell's project, as it stands in "Discourse analysis and social psychology" is that it doesn't show an interest in the potential for analysing discourse moment by moment that we encounter in conversation analysis.

It is ironic that they take this position because the very thrust of ethnomethodology, as a reaction to Parsonian structural theories is that it takes a cognitive, not a motivation solution to the problem of social order. I will elaborate on this in chapter eight. Furthermore in dealing with the meaning that phenomena have for members we are adopting a cognitive stance, we are looking at their categorizations, their understandings.

Drew (1986) draws on the phenomenon of letters crossing in the post to illustrate the idea of behaviour as involving shared, and in that sense, social characteristics. He suggests that both letter writers orient to a reasonable delay (since last contact), even without attempting to show how both parties calculate when to write, this demonstrated that both shared the same feelings about the delay. Drew then proceeds to a conversation analytic finding (since published as Jefferson 1989) that pauses during a word search are usually about one second, that is to say the pause is interupted by speaker or recipient at that point. He points out that in a case such as the following fragment, even though neither has come up with an object, both orient to the pause in the same way: They share the same interactional competence.

```
Emma:        We just had a vo:dka Barbara and I: just had a nice
             great double vo:dka and we're having a barbegued
                  (1.2)
Emma:   →    [U h:  ]
Bud:    →    [Some- ] Something?
```

Such a conversational fragment, Drew argues, shows a practical lay psychology in action. Far from being a second order characteristic, one person's understanding of another or their ability to take the attitude of the other is a public phenomenon. Such a perspective points to a rapprochement between the

interactional and cognitive perspectives since its analyses point to the non-individualistic nature of behaviour. In other words, there is an interactionally produced structure to the interaction which can be described at a social level not an intrapsychic one.

2.1 Rapprochement

In chapter one I noted Cicourel's suggestion that a "cognitive sociology" would explicate social phenomena in terms of members' knowledge to show how social structure is generated, taking social structure in conventional work to be an "accountable illusion". Here then, Cicourel points to not merely a rapprochement between a cognitive and a social approach but indeed a necessary union. However despite its relevance to my present concerns, Cicourel does not go the whole way since interaction itself does not become the detailed focus of his work.

Tannen and Wallet seek to use both "interactive frames" and "knowledge schemas" in their account of a doctor's talk in a pediatric interview. They claim that both ideas can be fruitfully used in parallel to describe what they suggest are general phenomena, but ones which are particularly clear in their data. In the interview which they discuss the doctor has to coordinate interaction with the child, the child's mother and with a video camera (a future audience watching the recording for teaching purposes.

Goffman suggested that the "interaction order" was a place for autonomous research with its own structure. On the other hand, I don't think that interactive phenomena can be thought of as pure emergence, obviously people bring things to the interaction scene: the interactions are the interactions of people with knowledge, social and otherwise.

3.0 The interactional dimension to forgetting

If the occurrence of one's mind "going blank" when an important question is asked is a familiar one then so is forgetting as an interactional phenomenon.

I want briefly to discuss some attention which as been made to this by conversation analysts.

A candidate non-cognitive stance appears in an interesting paper by Goodwin (1987), to which I referred in chapter four when discussing the relationship between speech and body movement. The datum comes from a "midwestern backyard picnic" where three couples are present. Mike is talking about something he saw on television:

```
Mike:    I was watching Johnny Carson one night
         en there was a guy by the na- What was that guy's
         name.=Blake?
```

Goodwin remarks that:

> Within both the psychodynamic and the speech processing approaches, such events are effectively treated as being situated within the mind of a single individual, the speaker. (p.115)

I'm not sure that the way that psychoanalysts understand the analytic situation entails Goodwin's interpretation but certainly the analysis which Goodwin offers is, I think, highly distinct from anything an analyst might say. Goodwin focuses on the consequences of this event as an interactional resource, showing the changes that come about in what he calls the participation framework as a result of it happening. I can't do justice to Goodwin's detailed analysis here, but briefly he discusses 1, how material being searched for is brought into prominence, 2, the organisation of participation during the word search, through this 3, the social identities invoked.

It should be pointed out that Goodwin's approach is not so radically anti-cognitive as it might appear. He takes the event as a "display of forgetting" and cannot of course, analyse its antecedents. He does not suggest that a memory failure occured which was socially caused as a floor winning strategy. The analysis has to take as its starting point an observable conversational phenomenon. Additionally the participants' knowledge is an important feature of his analysis since he considers the problem faced by a speaker designing talk for an audience with different knowledge.

Drew (1989) analyses a report of an inability to recall something and offers an explanation emphasising the importance of social factors for cognitive science. Victor, an elderly relative visiting from Germany is entering Ivy's appartment for her birthday party along with some other visitors. Ivy's daughter, Betty, who lives in the appartment with her mother is the speaker on whom Drew's analysis focuses.

```
Victor:   That's Betty.
          ((Betty and Victor shake hands))
Jim:      I haven't seen (          ) Debbie no w-
Victor:                  [Kennst du mich noch?=Do-
          do you- do you know me [:?
Jim:                     [hehh=
Ivy:      =See hehe[‘hh
Victor:            [°huh? Do you know me?
               (1.9)
Betty:    Unh (0.4) I don't know (h) (h) [ (whether I)
Jim:                                     [Uh let's go=
Betty:    remember [(you)
Jim:               [=inside 'n close the door.
```

This forgetting is puzzling at first sight, for it is certain that Betty knew that Victor would be coming and it will have been easy for her to pick him out. The reason for her forgetting is, Drew suggests, that she is responding to a specific understanding of what it is to recall someone, she is interpreting Victor's questions not as "do you know who I am" but "Can you recall me? Do you have an image of me from the past". Of course the answer to this must be no. Betty was very young when she last encountered Victor. To have such a recollection, to be able to say "That's Betty" is the prerogative of an adult.

A basic and important point that is raised here is that our everyday language for cognitive phenomena contains a range of meanings which refer back to a social realm.

Conclusion: Cognition or interaction

I've concentrated on the polarity between cognitive represention of knowledge and interactive accounts. It is my impression that perspectives such as attribution theory and personal construct theory would benefit from attention to how subjectivities are created, maintained and communicated and changed and that this points in the direction of interaction rather than represention.

Interaction involves the operation of a range of cognitive phenomena but there are two objections, I think, to seeing a cognitive account as primary. Firstly the interactive phenomena themselves need to be understood, secondarily the representational account, independently of whatever philosophical objections there are to it, and however "flexible" or "dynamic" it may be, can ossify how people in interaction might be studied.

CHAPTER EIGHT: CONCLUSION

When introducing the idea of constraints in communicative situations I stated that I did not wish to lose sight of the sense of a constraint as a coercive force. In studying explanations as social phenomena I have found that the central issues that concern me here have been handled in an interesting way by the ethnomethodologists and I have drawn heavily on their work, particularly in connection with conversation analysis. I want to conclude with some reflections evaluating the approach that I have adopted by 1, remarking on points of relevance for type of work carried out under the name of attribution theory and 2, developing some criticisms of the explanatory and descriptive power of the ethnomethodological approach by a consideration of the place where meaning fuses with power: ideology.

1,0 Ethnomethodology and attribution theory.

Though both have as their starting point a concern with everyday understanding this topic becomes embedded in research orientations of rapidly diverging character. To make some obvious points attribution theory restricts its subject matter by concentrating on understanding of causation whereas ethnomethodology, though certainly involving reasoning about causes, involves a whole range of issues of which causation is just a chapter. Secondly, such a statement of difference, though of course substantial, is utterly masked by the completely different understanding that students who formulate day-to-day reasoning as lay psychology have from those who formulate it as ethnomethodology. In connection with issues such as the expression of beliefs about causation in naturally occurring interaction there are a number of areas in the conversation analytic literature which are relevant, and hence address a project such as the coding of attributional statements.

1, The inductive style of work carried out in conversation analysis might be applicable to teasing out relationships of affiliation and distance amongst the members of a family, in particular by identifying the detailed way in which speakers respond to each other and what the interactional consequences are of

this. Such an approach might make concrete the interactional ideas that are widely held and applied by students of the family.

2, Attending to changes in the way in which things are formulated might tell us more about the nature of the beliefs that people hold. Construing beliefs as argumentatively held positions, as for example Billig has suggested (1987) would go some way towards acknowledging the integrity of people's beliefs and to understanding changes in them and acknowledge their situated character without adopting a position of situational determinism. Such a position is at least latent in the ethnomethodological idea of the "morality of cognition" which I sketched in chapter one. I hope to explain below how this also makes research of conduct - behaviour as meaningful action possible. Such a position could be one way of reconciling the apparent validity of the dimension of causes discussed in attribution theory with my finding in chapter two that a variety of styles are possible over and above the "depressive attributional style" and that people do not appear to hold fixed styles of attributing causes in these terms. Such an approach links up with:

3, Perhaps the most central area of interest to how therapy is conducted might be topic organisation. For this area explores a key feature of how the structure of a conversation constrains what can be said by the participants. Of related interest here is the way in which the precise formulations of some matter differ in their rectitude and a speaker's understanding of other's speech. This was a phenomenon which I tried to look at in chapter five, where in addition to the sheer fact that speakers routinely have command of a range of expression for some matter, I tried to point to the relevance of the conversational environment in explicating what such changes respond to and achieve. This idea links up with the remarks I made in one above. Rather than seeing changes in a speaker's way of apprehending some matter as situationally determined or haphazard studying the data with an eye to the speakers' concern with making sense of what is taking place and being intelligible in some way reveals a latent rationality.

4, In connection with attribution theory in particular a couple of papers by Pomerantz (1978b, 1986) show how the formulation of accounts in conversation might be of interest in studying causal beliefs. In her brief report "Attributions of responsibility: blamings" she shows how blaming is often done in turns

subsequent to one where an "unhappy incident" of some kind is reported. She suggests that this format may be organized in such a way as to allow blaming self rather than other. Such a study suggests a supplement to my simple survey of the range of causal expressions detected in a sample of talk (chapter three) by adopting an interactional perspective in order to look at the work done by different sequences of causal attributions. In "Extreme case formulations: a way of legitimizing claims" Pomerantz shows how formulations such as "everyone", "brand new", "he was driving perfectly" may be used:

> (1) to assert the strongest case in anticipation of non-sympathetic hearings,
> (2) to propose the cause of a phenomenon,
> (3) to speak for the rightness (wrongness) of a practice.

Pomerantz's analysis shows a striking relevance to the dimensions of causes which attribution theory has fastened itself to but rarely explored (to offer a moderate formulation) in naturally occuring interaction. She shows how a speaker can take what she calls an adversarial or defensive stance. In a study of a small-claims court a plaintiff formulates a dress which she claims was damaged by a dry-cleaner as not just "new" but as new as possible (brand new):

```
Adj:  And, you state that- the dress (.) was new
Pla:  It was brand new.
```

In attributing a cause to an object: Here a speaker is talking about some fruitcakes that she was selling. In accounting for why she priced and sold fruitcakes in the way that she did she invokes a description of a situation which is formulated as recurrent (every time):

```
C:    ...well I'm doing it to the few people that I know because
ever'time I say three twenty five they look at me like 'hh (.)
you must be nuts woman,
```

Frequency of occurrence in connection with rectitude. In the following fragment a patient has gone to get some laboratory results. When it turns out that they haven't come in the receptionist asks the patient to telephone a few days later. When the patient checks on the regularity of making such a call the receptionist describes such behaviour in terms of frequency (all the time):

Patient: That's not a problem?
Receptionist: No. People do it all the time.

A significant divergence between the conversation analytic approach and the kind of work to which attribution theory has been applied is that the latter has tended to be about exploring differences between people whereas such an approach in principally and methodologically distant from conversation analysis in so far as this fastens on recurrent patterns which are assumed to be general features of human interaction. The difference is perhaps not so great if two, rather different, considerations are taken into account. Firstly, CA has been applied to cases of a single episode in an interaction. Secondly CA, in taking interaction as its subject matter reformulates rather than negates considerations which would otherwise have been pursued as differences between individuals.

In the remainder of this essay I propose to focus on some criticisms which have been made of the perspective of ethnomethodology in order to examine the sense in which interactional phenomena are social ones and to explicate where I think the principles embodied in the practice of studying ethnomethodology lead.

2.0 Criticism of the formulation of social life as "ethnomethodology"

Two criticisms which are raised against ethnomethodology are that 1, the approach does not address the autonomous and traditional sociological concern of social facts and 2, the approach is relativistic and does not therefore equip the student of social life to criticise the phenomena that are studied. I think that these criticisms are interesting and important. That I consider them important is a value judgement on my part, but one that I hold since because I hold that any action is embedded in a backdrop which is always already there it follows that the idea of a neutral statement is problematic. I am therefore committed to the view that descriptions are prescriptions. Incidentally I think that this view is entailed by the principles from which ethnomethodology starts off - it is simply a reflexive application of them to the student of social life.

As I shall show below, some statements of ethnomethodology indicate not a relativist position but an absolutist one. It is however relativism that most frequently crops up as a target of criticism, for example Billig (1977).

Before I go any further with this I had better indicate what I mean by relativism. Relativism can be a powerful palliative for whiggism in the context of history and for chauvinism of other kinds in the social and cultural realms too. Relativism can occur in a variety of forms which should be distinguished. Primarily, relativism in the context of the social sciences involves a suspension of the student's own notion of truth and acceptability in order to give free reign to those of the people being studied. It is not difficult to see the moral grounds on which objections can be made to such a position. A relativistic study of some morally abhorent activity would not enable the student to call into question the acceptability of what is going on. Now such a position, though it may follow from the premises which I have coarsely sketched, is not inevitable. It depends on the strength of relativism embraced by the student. A methodological stance might detachedly try to reveal the intricacies of the situation and then having done that use such material in a criticism of that behaviour. The issue of relativism itself would seem to me to be independent of the adequacy of a research programme to take on morally problematic topics. However there is a principled variety of relativism which prohibits a student holding it consistently to adopt it as a research attitude but come back with a critical one. This would be the position that does not adopt relativism as a research attitude but as a moral principle. I think that there are desirable aspects to relativism but that these can be incorporated into a consistent picture which articulates how a student can simultaneously avoid adopting an imperious standpoint, that is remain sensitive to the concerns of the participants under study but at the same time be thoroughly and inescapably critical of it.

I will try and present such an account via a discussion of ideology, but first I briefly want to address the issue of ethnomethodology and social facts.

2.1 What about social facts?

I have already mentioned (chapter one) that the micro-sociologies in general, though contrasting with macro-sociology should not be indentified with methodological individualism so ethnomethodology does not involve a principled rejection of collective phenomena as subject matter, indeed I hope it is clear that conversation, for example is construed as a thoroughly social phenomenon. In so far as an implicit critique of macro-sociology can be found in ethnomethodological work it is as cautioning against making claims on the basis of decontextualised evidence. For example Smith (1974) reformulates the recipe for creating an ideology in "the German Ideology" and uses it to argue for attending to the importance of concrete, situational details of the social process under study. This is also the path that Cicourel takes in his study of how juvenile crimes are coded. The argument is, it seems to me, a double-edged one since it is problematic how, in general, the contextualisation of some research topic could be exhaustively reported, let alone studied.

Heritage closes a review of conversation analysis with the following interesting, and perhaps unexpected remark:

> However, it is also likely that the kinds of observation which are presently being made will both require and stimulate new theoretical work and it may well be that, since the ultimate object of investigation is a major dimension of social organisation, the explanatory theory will not be reducible without residue to the properties of rational agents. It may, after all, be unwise to exclude a Durkheimian sociological perspective from the analysis of what he himself pronounced as a prime instance of a social fact and from a domain in which institutionalised conventions are so intricately tied to the exercise of human agency.

But what could a research perspective which takes as its concern local details say about ideology? How could the meaning which members find and construct, which they offer accounts of and require of each other grasp a societal phenomenon such as power?

An opening response could be that precisely those local phenomena have to be invoked in an account of power or any other society wide phenomenon since whatever its pedigree as social fact it issues in, or is experienced through,

concrete datable events. Furthermore, language or meaning must surely play a role in such a phenomenon.

Sociologists have been alert to the role that language plays in the organisation of society, the role that language plays in the coordination of action. After all what is a society but a coordinated group? To understand a society would surely involve grasping just how action is coordinated, how communication operates. Anyone using the concept of ideology is in a certain sense plying this tack. To cite an ideology as an explanation for how a society does what it does is to draw on a model of a belief system within which that society's members formulate their strategies.

Barry Barnes's study The nature of power attempts to explain power in terms of the concrete practices of the members of a society. He repudiates a functionalist approach to power as a matter of principle. A functionalist account reasons in terms of structures in society on the analogue of a biological organ. Barnes dismisses such accounts as inherently teleological and holds that they must be abandoned. His point is that such an account makes appeals to mysterious forces, Foucault's account of power would be an example. What would an empiricist account of power look like?

Barnes develops his account of power from a consideration of the fundamental problem of social theory: the problem of social order. Hobbes sketched this problem as follows: how do people living in close proximity to each other and each pursuing their own ends not fall into a war of all against all? The American social theorist Talcott Parsons took the problem up and reasoned that the conceptual apparatus of sociology developed in Weber and Durkheim could furnish a solution. Barnes and incidentally Garfinkel both react against Parson's solution. I want to sketch Parson's motivational solution and the cognitive alternative to it.

Parsons rejects individualistic accounts of social order. Drawing on Durkheim's criticisms of nineteenth century economic theory he argued that individuals internalise norms through socialisation. Economic theory considers society as an aggregate of self interested individuals who interact in order to exchange goods

and services. Such interactions carry on because they are rational and profitable and hence society persists. Durkheim showed that any such interactions involve non-contractual elements; even the most basic economic transaction cannot be understood merely by reference to the means-end reasoning of the participants. As an example Barnes considers the case of ownership. Ownership is presumed by the economic account just sketched, but objects do not occur with given ownership labels tied by God's hands. Ownership is more puzzling than this. Any analysis of it must point to an interesting set of social relations within which economic transactions occur. Ownership of property is a shorthand for the discretion which a person has over the use of some object. Such discretion has to be profoundly social since it can be challenged by such things as commandeering. A person only owns something in so far as they are recognised by other people to have discretion over what they do with it.

Parson's solution to the social order problem is to seek an explanation for why individuals carry out actions which are orderly and consistent with social order. His solution is to claim that societal norms are internalised. This means that the desires to which agents apply their reasoning abilities and even the modes of reasoning themselves will be just those that are parallel to the persistence of an ordered society.

But there are insuperable objections to this "normative determinism". Barnes summarises:

> Norms and values are not implanted stably in individual minds; they persist in the public realm not the private, the social context not the individual psyche. Norms and values have no inherent implications which enforce and sustain a social order; on the contrary they are provided with implications by interacting human beings, so that what norms imply can in no sense explain how people interact. Accordingly, we can conclude without equivocation or qualification that normative determinism fails.

A cognitive alternative to Parson's motivational position can be introduced by claiming a priority for it. In his study of Garfinkel, Heritage argues that no appeal to motivational phenomena, however ingenious, can explain coordinated action, whether it be deviant or pro-social. Therefore a theory of social order

must be sought at the cognitive level. Barnes's method is to argue that the process of socialisation cannot be appealed to as the place where sociability is launched because socialisation is necessarily already social. Barnes seeks to establish this by remarking that in learning a child is trusting and cooperative. Incidentally, I don't think that such a claim is required. Barnes can be more parsimonious: all he needs to say is that socialization, whatever else it is, involves grasping a pattern of conduct, if you like, getting the hang of a form of life. It is irrelevant whether or not that form of life is knavish or virtuous. Barnes continues, in the style of Heritage, that it would not benefit an individual to deviate from established patterns of meaning - language is a solution to a coordination problem. And it is in the interests of members of a society to coordinate their actions. Now an individual may be knowledgable and calculating, but that knowledge and those calculating abilities are those of the society of which that individual is a member. On this account any calculation or use of knowledge is a kind of conformity: social order is the use of cooperative routines.

Having sketched the cognitive account of social order I want to look at Barnes's cognitive account of power. Considering society as a distribution of knowledge means that knowledge of norms is included. Barnes offers the example of obeying a traffic light on the basis of one's knowledge of what other people know.

From this we get to Barnes's redescription of power:

> A society is a distribution of knowledge, part of which knowledge is self referring knowledge of the nature of society itself: the self referring knowledge of the society includes the normative order of the society. Power, therefore, that is to say social power, must be an aspect or a characteristic of a distribution of knowledge, and indeed this is how I propose to define and conceive of it. Any specific distribution of knowledge confers a generalised capacity for action upon those individuals who carry it and constitute it, and that capacity for action is their social power, the power of the society they constitute by bearing and sharing the knowledge in question. Social power is the added capacity for action that accrues to individuals through constituting a distribution of knowledge and thereby of society.

In a sense Barnes is arguing that a group of people, or an individual has power because of their position in the distribution of knowledge. To put it bluntly, that they have power because they are believed to have it. He illustrates this by considering what a successful revolution does: it brings about a shift in knowledge. Although the Shah of Iran had lots of fire power, tanks and weapons and the mullahs had very little, the Shah had power only through a knowledge structure, not directly. On Barnes's account that is the nature of power.

In passing it seems to me that in Barnes's account, even if it is true that an account of power can be given at the level of detail of individual beliefs, the distribution of knowledge is demonstrably not the belief of any individual. I feel that an account of society at this level creates scope for an account in terms of social systems which in a sense may be functional. I don't want to go into this here.

On one level this kind of account is, however, clearly correct. However complex the collective or even merely aggregate action in a society is it is the result of people acting on the basis of the understanding of the situations that they find themselves in. One objection to the adequacy of such an account is that whilst it may be correct it is not appropriate for the study of such things for there is an emergent complexity which is best captured by some other formulation. Perhaps an analogy could be drawn between accounts of heat in the physics. Kinetic theory pitching its explanation in terms of the behaviour of particles held to constitute the volume of gas under consideration and thermodynamic theories which reason in terms of macro properties. The theory is however vulnerable to internal criticisms on the grounds that social power, as opposed to brute force, remains unanalysed in the case of a face-to-face interaction. Can the feelings that a person might have in a confrontation with an authority figure, or even the power holder who stands behind the power that is transmitted through bearers of authority, be wholly explained in terms of a cognitive model? I shall try to argue later that this criticism can be met without moving far from Barnes's premises.

2.2 Ideology

I want to discuss these criticisms via a discussion of the realm which more than any other must link them: ideology. I propose to do this by considering an account of ideology which at first sight must appear to be fundamentally at odds with anything that a student of social life in the ethnomethodological tradition might want to say: an account which tries to fuse the depth hermeneutics of Freud and Marx.

It may seem surprising that anyone should have attempted to integrate Freud and Marx but this was the ambition of one group of social researchers, the Frankfurt School, for whom the emancipation of society and the autonomy of the ego were inseparable. Freud is remembered for the development of what was for him a medical procedure for the treatment of certain psychological disorders. Although he spent his life in the treatment of patients psychoanalysis is now studied as a theory of mind as well as a method of therapy. In a way this reception is accurate. Psychoanalysis sees the disorders that it concerns itself with as due to conflict between parts of the mind; conflict which the sufferer is not insightful into but which finds confused expression in neurotic and hysterical symptoms, parapraxes ("Freudian slips"), and paradigmatically, in dreams. Discovering the conflict requires the interpretation of such phenomena. This interpretation is coordinated with a model of the adult mind and an account of its development through childhood. Drives enter the picture because Freud sees there being certain inevitable needs which people demand. Conflict results because these demands simply cannot be met. Freud came to suggest that demands flow from the id which operates according to "the pleasure principle" but are related to the world by a structure which grows out of it, the ego, which operates according to "the reality principle". The super ego later develops: it houses societal demands, which on this model are usually mediated through the child's parents.

Freud's model of the mind contains another tripartite division: conscious, preconscious and unconscious. The conception of the mind as split into an introspectible part and a part hidden from introspection is of course not Freud's invention. It is not difficult to see his reasons for taking on such a picture. As

a young neurologist he encountered cases of paralysis which could not be explained in terms of neurological disorder. The disorder must then, he concluded, have some mental cause. (See, for example, "Some points for a comparative study of organic and hysterical motor paralysis".) But if this is true then it must be mental in an extraordinary sense for the patients were not apparently willing the paralysis. Thus the idea of the force of an unconscious part to the mind became established. The "thoughts" here cannot be brought to consciousness by the person. The preconscious contains those thoughts which we may have, though do not happen to be aware of at the moment. The aetiology of other disorders as unconscious goings on followed; the classic case being the repression of traumatic events. The task of the analyst is to work out the nature of the repressed phenomena, hence the attention to the manifestation of unconscious phenomena and the need for interpretion. The way to cure is the abreaction of such repressed thoughts in the psychoanalytic session.

Freud was a learned man with a passionate interest in archaeology. He modelled the task of interpretation on classical philology and wrote a number of works relating to mythology and religion. In addition he treated of more or less sociological topics. The Standard Edition (vol. 13, p. 162) lists some twenty six works of this kind, for example, Totem and Taboo, Moses and Monotheism, Civilization and its Discontents, and The Future of an Illusion. The social theory which I want to consider does not follow Freud's lead here, it takes up the concerns at the very centre of psychoanalysis.

Marx offers an account of the evolution of human societies which takes as its guiding thread the way in which people produce things, that is labour. We may expect such an account to be fundamentally at odds with the perceptions of people in society, for societal change is not the result of individual intentional action. In his account Marx develops a critique of the contemporary social formation: the way society is now is not the only possibility. The structure of society, the organisation of the means of production, have resulted in the placing of certain values over others. His account of the development of capitalism and his criticisms of capitalist society has as an allied project the explanation of two related phenomena: how are the contingent social practices which are constitu-

tive of capitalism justified and how it is that people comply with them? Taking up these concerns is the business of a critique of ideology. We have an example of such a project in "The German Ideology" written by Marx and Engels in about 1844.

In common with Freud, Marx presents his work as not merely a theory but a tool for action. Although Marx deals with phenomena which are both individual states of mind and social phenomena, (for example "alienation") and although Freud locates societal demands in his account (the punishing power of the super ego) there seems little in common between the work of these two people.

One way of seeing Marx and Freud as engaged in related tasks is to adopt the perspective that both took the phenomena which they dealt with to be problematic in the important sense of demanding special critical tools for their elucidation. Both paid special attention to the complicity of the people involved. Both held that knowledge grasped through the methods they advocated would be emancipatory. In "the fetishism of commodities and the secret thereof" Marx examines the role of commodities in capitalist societies and claims that the categories of bourgeois economics are incapable of grasping the true nature of commodities. Contemporary economic self-understanding is placed in a model of societal development. As we have seen Freud develops a method of enquiry seeking to explain phenomena which cannot be grasped by the person in whom they are held to occur; the origins of such a failure of self knowledge being located in a phase specific model of individual development. Ricoeur (1970) has termed Marx and Freud (along with Nietzsche) masters of "the school of suspicion".

Habermas offers a reading of Freud which departs from the emphasis placed on the theory of drives by the earlier "Frankfurt School" theorists. He also differs from them in his concern with language. In his Knowledge and Human Interests, Habermas makes the point which Ricoeur made: that Freud moves beyond the conventional concerns of interpretation theory (hermeneutics), the explication of a text whose meaning is in some way unclear. Freud's step in considering dreams and behaviours as text-analogues with some confused

meaning is to insist on and to offer an account for the mechanism which produced the distortion. From this reading of Freud Habermas (1970a) develops the idea that neurotic behaviour is systematically distorted communication. That is, behaviour distorted at source, not at the stage of transmission but at the stage of production. In contrast to archaeological fragments, where interpretation is essentially about the production of a translation lexicon and the discovery of grammatical rules, the psychoanalytic patient is seen as the victim of distorting forces which operate within themselves, forces which have power precisely because they are not seen through. This account of the operation of such unseen powers, powers which the patient cannot articulate yet which dominate his or her life suggest a paradigm for explaining ideology. Habermas suggests that we construe ideology as the systematic distortion of communication at the societal level.

Ideology involves the legitimation of a certain system of norms. What is pernicious about ideology is that it inhibits thought, not merely that it places certain norms over others but that it somehow precludes their dethronement from the privileged positions which they occupy. The claim is that ideology somehow prevents rational communication within society. The goal of psychoanalysis is to free its patients from the repetitive patterns of thought and behaviour in which they are trapped through the re-integration of the mind. The ways of being which the patients took up, ways which they could not understand, are made to be mysterious to themselves no longer. They can now live openly with themselves; their minds now transparent to their own reflection: the patient is now free to act rationally. "Where there is id, there shall be ego."

There are at least two problems in trying to base an account of ideology on such a picture, or some variant of it. Habermas has to respond to the following two problems. Firstly it is by no means clear that a rational society, should such a thing be possible, would share analogous benefits to those which are claimed for the fully analysed person. Weber reminds us that the increased rationalization of public life during the enlightenment had negative effects. Life became increasingly bureaucratized. The exorcism of superstition brought about disenchantment in the sense of making the world spiritless, emptied of meaning.

Secondly the methodology by which analysis reaches its depth interpretations is far from unproblematic: how can we be sure that the construal of utterances as distorted by unseen power is a project free from correlated problems, that is, can Habermas justify the particular critique of ideology which he professes?

Habermas argues that Weber's view of rationality is too narrow. He claims that Weber restricted himself to means- end rationality, failing to note that the human ability to communicate - itself a prerequisite of society - reveals "communicative rationality". In his inaugural lecture he stated:

> The human interest in autonomy and responsibility is not mere fancy, for it can be apprehended a priori. What raises us out of nature is the only thing whose nature we can know: language. Through its structure, autonomy and responsibility are posited for us. Our first sentence expresses unequivocally the intention of universal and unconstrained consensus. (1972, Appendix, p. 314)

The claim is that the fact of language shows that humans have the ability to achieve consensus, to acknowledge each other as rational beings. But doesn't language also reveal our capacity for deception? Does every utterance aim at unconstrained consensus, what of jokes, deception, deliberately cryptic utterances, formal styles of speech, cant, technical language? Doesn't Habermas' claim that autonomy and responsibility are not utopian dreams itself rest on a romantic fiction? Habermas could parry this by claiming that the orientation to a shared understanding is the basic ingredient of speech and that although speakers clearly deviate from this in specific circumstances this fact does not affect his claim that "I take speech aimed at reaching understanding to be fundamental." I'll defend this in a preliminary way now by remarking that intentional deception requires insight into sincere communication. That is, far from having nothing to do with unconstrained consensus, deception has to utilize the possibility of consensus precisely to ensure that it remains latent. Habermas talks of such abilities as communicative competence. That is the ability to place utterances in appropriate contexts: to use language. We may contrast this with the narrower scope of Chomsky's idea of linguistic competence which is the mastery of an abstract system of rules. Habermas refers to a research programme called universal pragmatics which aims at the construction of the requirements of

communicative competence. It is universal in the sense that it deals with abstract properties of communicative acts, properties which are held to operate in any speech situation whatsoever. The starting point for this project are presuppositions which Habermas claims all speakers make:

> I shall develop the thesis that anyone acting communicatively must, in performing any speech action, raise universal validity claims and suppose that they can be vindicated {or redeemed}. Insofar as he wants to participate in a process of reaching understanding, he cannot avoid raising the following - and indeed precisely the following - validity claims. He claims to be: a. Uttering something understandably; b. Giving {the hearer} something to understand; c. Making himself hereby understandable; and d. Coming to an understanding with another person. (1979)

I think that the critical potential of this position can be illustrated vividly by contrasting Habermas with the position adopted by students of ethnomethodology. In so far as the ethnomethodologists argue that all that sociology can have as its subject matter is the everyday practical reasoning processes which people engage in, talk of 'ideology'; 'power' - the traditional concerns of academic sociology - are dismissed as reification. They are only of interest to sociology, the ethnomethodologists argue, in so far as people use them to structure their experience and organise their actions. Like the ethnomethodologists Habermas notes that there is a "given" background in our understanding of the world, an "always already". But a sharp difference emerges when we look at how communication is to be construed. For the ethnomethodologists, claims to rationality or universality by people in conversations are not universal, they are merely taken to be universal (Garfinkel, 1967, p.33). Habermas responds to this by pointing out that if the ethnomethodologists really want to follow this through then their own observations of people in conversation have a strange status. The ethnomethodologists could claim that their observations have no theoretical status at all, and are merely the local management of actions of a bunch of academics. The alternative is to claim that their observations have a status beyond the mundane because the ethnomethodologist has a disinterested position and can watch how the participants go about making their claims comprehensible

(Garfinkel and Sacks 1986, p. 166) But this is to claim that the ethnomethodologist enjoys some standards of validity beyond ordinary speakers. No such problem for Habermas. If we hold that speech involves making universal validity claims then Habermas can state:

> The social scientific interpreter, in the role of an at least virtual participant, must in principle orient himself to the same validity claims to which those immediately involved also orient themselves; for this reason, and to this extent, he can start from the always implicitly shared, immanent rationality of speech, take seriously the rationality claimed by the participants for their utterances and at the same time critically examine it. In thematizing what the participants merely presuppose and assuming a reflective attitude to the interpretandum, one does not place oneself outside the communication context under discussion; one deepens and radicalizes it in a way that is in principle open to all participants. In natural contexts this path from communicative action to discourse is often blocked; but is always engrained in the very structure of action oriented to reaching understanding. (1984, p. 130)

Before commenting on this position I want to pause to consider the mechanism which Habermas appeals to in order to argue that movement into the realm of what he calls "discourse" secures a special validity. What has yet to be shown is how entry into a dialogue could lead us to the truth as opposed to say, some endpoint secured by the slick dogma of a middle class intellectual critical sociologist, that is, the operation of yet another ideology. Here Habermas introduces a consensus theory of truth. When truth claims are called into question they can be settled only through argument. Consensus here cannot mean any contingently reached agreement. What is required is a notion of a justified consensus. What could such criteria be? To find out Habermas argues that we must go back to examine the very structure of discourse itself - at the very notion of providing rational grounds for a belief.

If we enter into a discussion with a view to reaching an agreement (a rational agreement) then we must suppose that the agreement is to be reached without coercion. We must suppose that the result will be reached through force of better argument alone. Such discourse is clearly an ideal case, nevertheless there is merit in looking at what would have to be the case for such an outcome to be possible. Such a discussion would require equal chance for all the

participants to raise claims and call claims into question, equal chance to perpetuate discussion. Habermas calls discourse having this structure an ideal speech situation. Where Chomsky considers an idealisation of a speaker's competence; a speaker without constraints on memory, who has the capacity and attention to understand infinitely long sentences, Habermas considers an ideal situation where consensus worthy of the name can be reached. Of course this is a fiction. Power is usually mobilized to win arguments, the way to consensus is usually blocked. For Habermas these are contingent issues; due to, and constitutive of, the society which we live in. Habermas' idealisation must be sharply distinguished from Chomsky's. The ideal speech situation results from the structure of speech: it is a reciprocal requirement of speakers when they act with the intention to communicate, it is neither an empirical finding nor an approximation made for the sake of a research programme. Habermas claims it is anticipated by all speakers. "This anticipation alone is the warrant that permits us to join to an actually attained consensus the claim of a rational consensus. At the same time it is a critical standard against which every actually realised consensus can be called into question and tested." (xviii, 1973)

2.3 Implicit rationality and universal rationality

The difference between Habermas's position and the ethnomethodological train of thought (I use this expression because of course I am constructing a position based on their views rather than reporting) is interesting because as I noted they do actually start from standpoints which are similar in a crucial respect. The idea that human conduct is always situated in a context which is already established and which is renewed by such conduct is a shared idea. For both theories the phenomenological idea of the "lifeworld" is drawn on in articulating this. Habermas, in the second volume of The theory of communicative action qualifies the adequacy of such a position, linking up with the remarks which I made above in connection with social facts. I shall not attempt to take this further. Furthermore both approaches allocate rationality, in a wider sense than the term often has, a place of fundamental importance.

I propose to point out a way of reconciling the ideas on truth which Habermas and the ethnomethodologists propose. That they should be reconcilable is

something which in principle seems possible to me for both incorporate what strike me as profound points about human interaction. On the one hand, Habermas's position exhibits an idea which I consider fundamental to the concept of truth:

(a) Whatever else truth may mean, if a proposition is true then everyone, in principle assents, to it.

On the other hand the ethnomethodological position also makes a fundamental assertion about truth:

(b) What is taken as true is ineluctably tied to a determinate context, a certain "here and now".

So on the one hand truth claims are universal, but on the other they are local. I shall argue that these positions are but opposite sides of the same coin. Firstly, whether or not a truth claim is local or not depends on one's view of language. I think that the only intelligible interpretation of Habermas's position has to acknowledge that in so far as an expression only means something determinate in a given context then to be sure truth is highly local in that sense. I am sure that Habermas must hold such a view.

As Davidson remarks, the mandate for interpreting someone's speech can be thought of as holding a theory of what their words mean. Any theory of interpretation presupposes rationality on the part of the person being interpreted, since without the assumption of a shared pattern there could be no consistencies within which the attribution of beliefs to a person or the interpretation of their utterances could start. Stressing the contingency of whether or not an interpretation can be reached and a rationality discerned is the ethno-methodological position. This is a sceptical version of Habermas's position. The perspective adopted is that the local rationality is presupposed, a condition for the action having a meaning at all, but the generality of claims made by the participants are denied since their rationality, prior to critical engagement by the investigator, is what is sought. These two subtlely but importantly different positions are, as I understand them methodological bases. I think that it can be shown that the ethnomethodologist subscribes to a version of the thesis that speech has a universal validity base since the theme of the accountability of

conduct suggests that any action is accountable to an indefinite range of demands for an account. Furthermore, it seems to me impossible that a person could design some conduct to be specifically unaccountable. So the universal validity basis of speech has its counterpart in the accountability of conduct. Indeed the fact that ethnomethodological studies occur at all indicates that students of ethnomethodology reckon that there are at least reasonable odds of an interpretable action taking place, that is, one whose rationality can be grasped.

2.4 Social power and communicative constraints

In the light of the idea of the 'ideal speech situation' Habermas's appeal to a psychoanalytic account can be re-evaluated. The fact that the ideal truth situation is counterfactual means that it requires an account of communication distortion as its counterpart.

At this level of I think it transpires that the appeal to a psychoanalytic account is rather abstract. It amounts to the claim that 1, there are internal barriers to communication, which fulfills the requirement that ideology as a limit on what is thinkable is accounted for, and 2, that the distortions are depth-hermeneutic in nature, that is to uncover the mechanism producing the distortion is to exorcise it.

It is ironic that whilst Habermas visualises a profoundly social sense of rationality the account of ideology as distortion as I have sketched it here is oddly individualistic. Of course an unplayed card is the two sided nature of the psychoanalytic formulation itself. Within this account intrapsychic goings on have a social dimension in so far as they are modelled on, and, on this account, have their genesis in, relationships with other people. Despite this, this whole account represents a formidable obstacle to reconciliation with the concepts of ethnomethodology. What could an ethnomethodological account of ideology be like?

Earlier I criticised Barnes for failing to address the communication of the knowledge on which his account rests. Latent in this account, however, lies a suggestion which can relate the normativity of knowledge, communicative constraints and power, in short it can furnish an account of ideology. If the knowledge in Barnes's account includes communicative knowledge, or better, communicative know-how then the meanings written into language, since the meaning of a word is socially defined, are the basic ones within which values are contained. To use language or engage in interpretable activity is to exercise one's social knowledge, so whenever a person speaks or interprets what is going on they do so on the basis of their knowledge, their knowledge of the meaning.

This can, I think, go some way towards explaining an apparent difference in the constraints which I discussed in chapter four. The constraint which a question places on possible answers has a logical character to it whereas the "constraint system" impinging on someone responding to a compliment, though patterns of inference are clearly relevant has a much more normative character. Adopting the view that patterns of inference are social draws together the constraints evident in logical reasoning with those relating to giving offense or passing as a member of a certain group. They are to do with practical knowledge of how things are. Which, given the creativity required in the application of such knowledge and the capacity to criticise which goes with reason, could also be knowledge of how things might be, and even how to make them so.

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Appendix 1: Computional Procedure used for the studies of attributional style in chapter two.

The computing procedure which I finally used to calculate the binomial probabilities involved a SAS program. I am indebted to Paul Nicholson of the computing services for writing the core of this program. The original routine consisted of of three parts. 1, calculation of the frequency of occurrence of each attribution pattern, 2, calculation of the frequency required for that to be significant, 3, a comparison of the observed frequency with the critical value.

1, An SPSSX command file which picks up a file of codings, defines them and calculates the frequency of each style considered as well as each dimension separately. The frequencies are then cross-tabulated by speaker. This part therefore shows how many attributions of all the patterns possible within a given model each speaker has. The result is thus a table which is 2^n columns wide, where n is the number of dimensions in the model being investigated. The table is as deep as there are people involved. In the study the table are therefore either 32 or 8 columns wide and either 24, or typically 77 rows deep. Note that under some of the selections, for example considering only negative outcomes, some of the speakers with few attributions overall may have non at all.

2, The second part calculated the critical values for the number of cases of a given combination of the model under required for it to be called a style. Since the critical value, call it r_c, is a function of the probability of that combination occurring and the number of attributions present for that person a new value needs to be calculated for each combination for each person. I have taken as the probability for each combination the product of the proportions of each component dimension from the group of subjects as a whole. This means that any styles shown are significant differences within that group. I wrote a BASIC program which calculated the critical value of r at both the 5% and 1% levels as a two tailed test. Given the starting probabilities the program takes each person, cycles through all the combinations possible giving the lower bound critical values and then continuing with r+1 until the upper bound values are reached. In the case of a five dimension model for 77 subjects the program yields 32×77=1474 sets of critical values. (The program is written in Waterloo Basic).

```
00010 DIM S(77)
00020 LET S(1)  =     6
00021 LET S(2)  =    16
00022 LET S(3)  =     4
.
.
.
00094 LET S(75) =     5
00095 LET S(76) =     6
00096 LET S(77) =     0
00210
00220 LET NN = 77
00230 OPEN #6, 'BIN', OUTPUT
00240 LET Q =0
```

```
00250 LET C =0
00260 FOR J = 1 TO NN
00270   FOR PRY = 0 TO 7  STEP 1
00280     GOSUB 880
00290
00300     IF PRA = 1 THEN LET PRA = 0.600 ELSE PRA = 0.400
00310     IF PRB = 1 THEN LET PRB = 0.625 ELSE PRB = 0.375
00320     IF PRC = 1 THEN LET PRC = 0.311 ELSE PRC = 0.689
00350
00360     LET P = PRA*PRB*PRC
00370     LET N = S(J)
00380
00390     LET Q = 0
00400     LET C = 0
00410     LET Z = 0
00420     LET R1L = -1
00430     LET R5L = -1
00440     LET Q1L = -1
00450     LET Q5L = -1
00460       FOR R = 0 TO 50 STEP 1
00470        LET E = R*LOG10(P) + (N-R)*LOG10(1-P)
00480        LET X = N
00490        GOSUB 780
00500        LET F = C
00510        LET X = R
00520        GOSUB 780
00530        LET G = C
00540        LET X = (N-R)
00550        GOSUB 780
00560        LET H = C
00570        LET M = F - G - H + E
00580        LET T = 10**M
00590        LET QQ = Q
00600        LET Q = Q + T
00610        IF Z >= 1 THEN GOTO 650
00620        IF Q <= 0.005 THEN LET Q1L = Q
00630        IF Q <= 0.005 THEN LET R1L = R
00640        IF Q > 0.005 THEN LET Z = 1
00650        IF Z >= 2 THEN GOTO 690
00660        IF Q <= 0.025 THEN LET Q5L = Q
00670        IF Q <= 0.025 THEN LET R5L = R
00680        IF Q > 0.025 THEN LET Z = 2
00690        IF Z >= 3 THEN GOTO 730
00700        IF Q >= 0.975 THEN LET Q5U = Q
00710        IF Q >= 0.975 THEN LET R5U = R
00720        IF Q-T >= 0.975 THEN LET Z = 3
00730        IF Q >= 0.995 THEN LET Q1U = Q
00740        IF Q >= 0.995 THEN LET R1U = R
00750        IF Q-T >= 0.995 THEN GOTO 980
00760        GOTO 860
00770        REM WORKS OUT LOG10(X)
00780        LET C = 0
00790        LET A = X
00800        FOR K = 1 TO X STEP 1
00810          LET B = LOG10(A)
00820          LET C = C + B
```

```
00830          LET A = A - 1
00840        NEXT K
00850        RETURN
00860      NEXT R
00870      GOTO 980
00880      LET PRA = IP(PRY/4)
00890      LET PRX = REM(PRY,4)
00900      LET PRB = IP(PRX/2)
00910      LET PRC = REM(PRX,2)
00960    RETURN
00970
00980          PRINT #6, N;R1L;R5L;R5U;R1U;Q1L;Q5L;Q5U;Q1U
00990   NEXT PRY
01000 NEXT J
01010 CLOSE #6
01020 END
```

3, The final stage decides whether or not the number of each pattern worked out in stage one has only a 1% chance or a 5% chance of occuring. Such patterns are considered to be attributional styles. In effect, this stage compares the results of the each entry in the cross-tabulation table with the critical values. This results in a file showing the patterns which were significant, which speaker produced it, how many attributions they made altother, what the critical value is and how many of that pattern they produced.

To achieve the results presented in the chapter this routine had to be run separately for each of the selections, negative outcome, where the speaker is the target and so on. This meant running it twelve times. I used a mainframe computer to carry out all these operations. BASIC is not really the appropriate language for this kind of operation and the procedure is slow requiring a lot of computer central processing time. It was mandatory to run each job BASIC program as a batch job. Another problem is that having separate procedures means that several files are in play at any one time: the procedure is messy and the chance of error must be high and is difficult to check.

This is a listing of the SAS program written by Paul Nicholson and modified by me.

```
cms fi inl disk jprl dat;
cms fi out disk ray sas;
cms fi outl disk ray saslog;
cms fi outp disk ray output;
* read raw data and compute score;
data raw;
  infile in1;
  input        sp 1 @2  (x1-x5)(1.)
                                      goodout 7 sigout 8
         agent 10 @11 (a3-a5)(1.)
        target 15 @16 (t3-t5)(1.)
       speaker 19-20;
 score=10000*x1+1000*x2+100*x3+10*x4+x5;
  if target=. then target=sp;
  if sp^=target then delete;
```

```
   if goodout^=0 then delete;
run;
*proc print;run;
* compute empirical p values and store in data set empps;
proc summary;
  var x1 x2 x3 x4 x5;
  output out=empps mean=p1 p2 p3 p4 p5;
run;
*proc print;run;
* generate r values from tabulation - save in data set rvals1;
proc freq data=raw;
  tables speaker*score/noprint norow nocol nopercent
                       sparse out=rvals1;
run;
*proc print;run;
* delete empty cells;
data rvals2 (keep=speaker score count rename=(count=r));
  set rvals1;
*  if count > 0;
run;
*proc print;run;
* generate row totals separately and store in data set nvals;
proc summary data=rvals1;
  var count;
  by speaker;
  output out=nvals sum=n;
run;
*proc print;run;
* Need to identify all unique score patterns;
proc sort data=raw;
  by score;
run;
data scores;
  set raw;
  by score;
  if first.score;
run;
*proc print;run;
* now compute composite P value for each unique score pattern;
data pscores;
  if _n_=1 then set empps;
  retain p1 p2 p3 p4 p5;
  array p(i) p1 p2 p3 p4 p5;
  array x(i) x1 x2 x3 x4 x5;
  set scores;
  pscore=1;
  do i=1 to 5;
     pscore=pscore*(p*x+(1-p)*(1-x));
  end;
* put p1 p2 p3 p4 p5;
run;
*proc print;run;
* merge r and n values for each speaker into data set rn ;
proc sort data=rvals2;
  by speaker;
run;
```

```
proc sort data=nvals;
  by speaker;
run;
data rn(keep=speaker score r n);
  merge rvals2 nvals;
  by speaker;
run;
*proc print;run;
* merge P value with result and save in RNP;
proc sort data=rn;
  by score;
run;
proc sort data=pscores;
  by score;
run;
data rnp;
  merge rn pscores;
  by score;
run;
*proc print;run;
proc sort;
  by speaker;
run;
* now compute binomial probabilities;
* for r-1 - to give top end;
data ufinal;
  set rnp;
  rb=r-1;
  if rb>=1 then ubprob=probbnml(pscore,n,rb);
* put speaker score ubprob pscore r n;
  run;
*proc print;run;
* select small tails;
data siggs;
 set ufinal;
 if ubprob >=0.995;
 file print;
 put speaker 1-2 score 4-9 @ 11 ubprob @ 25 pscore r 38-40 n
42-46;
*print frequency with which diff. sig models occurred;
  proc freq data=siggs;
  tables score;
run;
proc print;run;
* now compute binomial probabilities for lower end;
data final;
  set rnp;
  bprob=probbnml(pscore,n,r);
* put speaker score bprob pscore r n;
run;
data sigls;
  set final;
  if bprob <0.005;
  file print;
  put speaker 1-2 score 4-9 @ 11 bprob @ 21 pscore r 34-36 n
38-41;
```

```
  run;
***print lower end frequencies;
proc freq data=sigls;
tables score;
run;
proc print;run;
```

The advantages of the SAS routine are considerable. Primarily it is more reliable as it operates directly on the raw data. In addition it is much faster. On a portion of the data which I knew to be accurately submitted to the old routine the two methods yield exactly the same results.

Appendix 2: OCP Program to select certain candidate causal connectives in chapter three.

The program shows the words and phrases which were selected.

```
*input
references cocoa.
*words
ignore '! ? " :' _   :: := { } ^ \ / ;  '.
ALPHABET "` A=a=a4=a5 B=b C=c=c5 D=d E=e=e1=e2=e3=e4
F=f G=g H=h I=i J=j K=k L=l M=m N=n O=o=o1 P=p
Q=q R=r S=s T=t U=u=u1 V=v W=w X=x Y=y Z=z
@ ~ a1 0 1 2 3 4 5 6 7 8 9".
padding '((  )) [ ]  * + '.
*action
pick words "COS BECAUSE SINCE SO FOR AS THEREFORE WHY"
or phrases "DUE TO", "IN ORDER TO", "BY VIRTUE OF", "IN THE
LIGHT OF",
"IN VIEW OF", "OWING TO", "DUE TO".
do concordance .
maximum context 50 letters.
REFERENCES T = 6, A = 5, S =5.
*FORMAT
REFERENCES LEFT.
*go
```

Appendix 3: Transcription Notation

Vocal features

Particularly in Chapter five, but elsewhere too, I have used the transcription notation devised by Gail Jefferson. As I discuss in chapter four, where I introduced the approach of conversation analysis within which this method was developed, the notation is oriented to capturing the sequential structure of spoken discourse.

Goodwin (1981) warns his readers that transcribed fragments require reading in a special way and are as different to ordinary text as tables and graphs. The following is based on Atkinson and Heritage (1984).

Transcribed fragments usually start with a reference entry. Turns are written across the page continuing onto to a new line below if necessary. Turns by new speakers follow below. The left hand margin names the speakers, followed by a colon. Line numbers, to enable reference to details in the fragment, may be added to the left margin. An arrow may be placed in the left margin to indicate some detail. Reporting of part of a conversation may be shown, as is usual elsewhere, with three stops spaced horizontally to show that material has been omitted from an utterance. Where an utterance is, or utterances are, omitted three dots spaced vertically may be used.

Standard orthography is used with modifications intended to capture aspects of pronunciation. This practice has been criticised by linguists, and indeed must appear shocking to anyone with expertise in phonology. Nevertheless, given the amount of material routinely transcribed and the analysis to which it is put the convention seems adequate to me. The transcript should be viewed as a tool for either helping analysis along or reporting findings. Nothing here precludes detailed phonological analysis and indeed some very interesting findings have been made through attention to such detail (Local and Kelly, 1989).

A range of symbols are included, some of which take on a novel use, as follows:

1, Overlapping utterances '[' and ']' are placed at the start and end of the overlapping portion.

2, Simultaneous utterances '[['

3, Latched utterances '=' placed at the end of an utterance and at the start of the next one when the second follows the first with no gap.

4, Timings '(0.0)', (.), (-----)
Silences and pauses are times to tenths of a second and shown in parentheses. Untimable pauses, in practice, less than two tenths of a second are shown by a a stop in parentheses, '(.)'. This is often called a "micro pause".

As an alternative to giving the time as a figure the number of tenths of a second may be shown by giving that number of dashes in parenthesis. This

method is useful when nonvocal activity is to be recorded. For example, a pause of six tenths of a second may be shown as '(0.6)' or as '(------)'. When using this method, every tenth dash, that is each whole second is marked by a '+'. So, for example, '(1.1)', '(----------)' and '(---------+-)'
are all equivalent.

5, Aspects of speech delivery
Punctuation marks are used <u>not</u> to show grammitical features but to capture the way something is spoken. Goodwin (1981, p.48) remarks that this is not a new convention. Apparently, commas, full stops and question marks were used to show pitch changes in a rule for liturgical recitation in medieval Munster.

'.' stopping fall in tone
',' continuing intonation
'?' rising inflection
'¿' weaker rising intonation
'!' animated tone
'-' abrupt cut off

'↑' rising shift in intonation
'↓' falling shift in intonation

T: I am however (0.2) very ↓fortunate
(0.4) in having (0.6) a ↑mar:vlous
dep↓uty

'_' underscoring show emphasis

Capitalisation shows loudness
Degree signs show quietness °°

'hh' aspiration
"hh' inhalation

'gh' guttural quality

Subscribed dot shows hardening

'>','<' Flank part of an utterance to show that it is quickened.

6,
Double parentheses contain descriptions of aspects of speech or details of the scene, or of vocalisations which can't be captured easily.

Single parentheses containing material other than numbers or dashed, used to show timings as discussed above, are used to show uncertain hearings. Mulitiple

possibilities are shown one above another. Empty parenthesis show that a hearing of the material couldn't be achieved.

Non-vocal features

When I look at nonvocal goings on in chapter six I recorded details following extensions to the Jefferson system described above which have been used by Goodwin and Heath. Basically, nonvocal features are added onto a transcript of vocal ones.

Usually speaker's activities are shown above the utterance and recipient's below. Gaze at other is shown by a solid line parallel to the speech with which the gaze is simultaneous. Transition to gaze at other is shown with a series of stops, onset of gaze marked with an 'X' and dropping of gaze with a series of commas.

Other details may be shown by square brackets parallel to the text with which they correspond with verbal desriptions.

In the environment on which chapter six is based, a computing advisory centre, where an advisor and a user sit side by side at a desk I found it usefull to extend the gaze details to include gaze at printed matter, on the desk, and gaze at computer screen.

~~~~~~~~~~ gaze at screen
========== gaze at desk.
~~~~~~~~~~

Appendix 4: Letter sent to the head of computing services

THE UNIVERSITY OF LEEDS

Department of Psychology
Leeds LS2 9JT
United Kingdom
Telephone (0532) 431751

Head of Department:
Professor A. J. Chapman

3rd March 1988

Dr. A.A. Hock
Computing Services
University of Leeds

Dear Dr. Hock

As part of my doctoral research on communication I would like to make, and analyse, video recordings of consultations in the User Access advisory. Like all studies taking place in the Psychology Department I have submitted a proposal to our Departmental Ethics Committee. The project which I propose has been considered and approved; so I am writing to you now to ask your permission.

I propose to be in the advisory for about ninety minutes per day with advisors who have agreed to take part. I would like to record on about ten days. Users entering the advisory would be handed a brief explanation of the study and given the opportunity to decline. The recording would therefore not be surreptitious: I would mount a portable video camera on a tripod in the corner of the room. I would seek to cause a minimum of obstruction to the operation of the advisory and to the progress of the consultations.

Much of the work in my field has used somewhat artificial data, often from rather contrived situations. In my work I am trying to break from this. Briefly, I am interested in the structure of conversations and constraints on communication. I have recently been working on video recordings of doctor-patient interactions from St. James' University Hospital.

I am keen to start the project as soon as possible. Please let me know whether or not you see any difficulties or have any objections. Should you consent, please let me know whether or not I may consult the advisory staff directly myself.

I must emphasise that I am not a "social skills" researcher and I do not seek to cast any judgement on the advisory staff, or the users.

Yours sincerely

J.P. Rae

I fully support their application. The work is likely to have important implications.

Peter Stratton
(supervisor)

Appendix 5: Explanation handed to users of the advisory

Department of Psychology
Leeds LS2 9JT
United Kingdom
Telephone (0532) 431751

Head of Department:
Professor A. J. Chapman

As a part of my PhD research on communication I am making and analysing video recordings from a variety of situations. In the past, research in my field has tended to use data from rather artificial situations; I am trying to break from this in my work.

Please let me know if you are prepared to me let video-record your consultation session. The material will only be used anonymously.

Are you prepared to allow me to give other researchers access to the recordings, to the soundtrack alone, or to transcripts?

I must emphasise that I am not a "social-skills" researcher; I am not interested in passing judgment on you or the computing staff.

J.P. Rae

Appendix 6: Summary details of advisory sessions discussed in chapter 5

Day	Session code	Approx. time	Advisor code	Some details of the users	
1	UAA.1	3:30	1	man	home
	UAA.2	5:45	1	man	home (me)
2	UAA.4	3:00	2	man	home (me)
	UAA.5	6:15	2	man	
3	UAA.7	1:15	3	man	
4	UAA.8	22:30	4	man	
	UAA.8i	1:00	4	woman	home
	UAA.8ii	1:00	4	man	home
	UAA.9	15:15	4	man	
5	UAA.10	7:00	1	man	home
	UAA.11	3:30	1	two women	home
7	UAA.15	5:30	3	woman	home
	UAA.16	1:00	3	man	
	UAA.18	4:30	3	woman	home
	UAA.19	5:30	3	man	
	UAA.20	7:30	3	man	home
	UAA.21	15:30	3	man	home
	UAA.21i	1:30	3	woman	home
8	UAA.22	11:30	2	man	
	UAA.23	1:15	2	man	home

This table is intended to indicate how different advisors correspond to the sessions reported in the chapter and to show the range of times that the consultations take. Although my analysis says nothing of the sex or country of origin of the speakers I have given some details in case they are felt to be important since I have not assigned names in the transcripts which would indicate such things. I have only marked speakers as "home" if I am confident that they are "home students". I made no formal attempt to find out where the users were from or whether or not they were native speakers of English. The advisors were all men, with the exception of the second advisor who is called in UAA.20. The days on the left-hand side simply show occasions when I went to the advisory and came away with a recording.

Appendix 7: Summary details of advisory sessions discussed in chapter 6

Day	Session code	Approx. time	Advisor code	Some details of the users	
2	UAA.3	7:00	2	man	home
3	UAA.6	10:45	3	man	
	UAA.8	22:15	4	man	
6	UAA.14	11:00	5	man	home
8	UAA.15	5:30	3	man	home
	UAA.17	15:00	3	man	home
	UAA.19	5:00	3	man	
	UAA.20	7:30	3	man	home
	UAA.21	15:00	3	man	home
8	UAA.22	11:30	2	man	